PROCEEDINGS OF THE ROYAL SOCIETY.

SERIES A, VOL. 81, Appendix, 1908.

# OBITUARY NOTICES

OF

# FELLOWS DECEASED.

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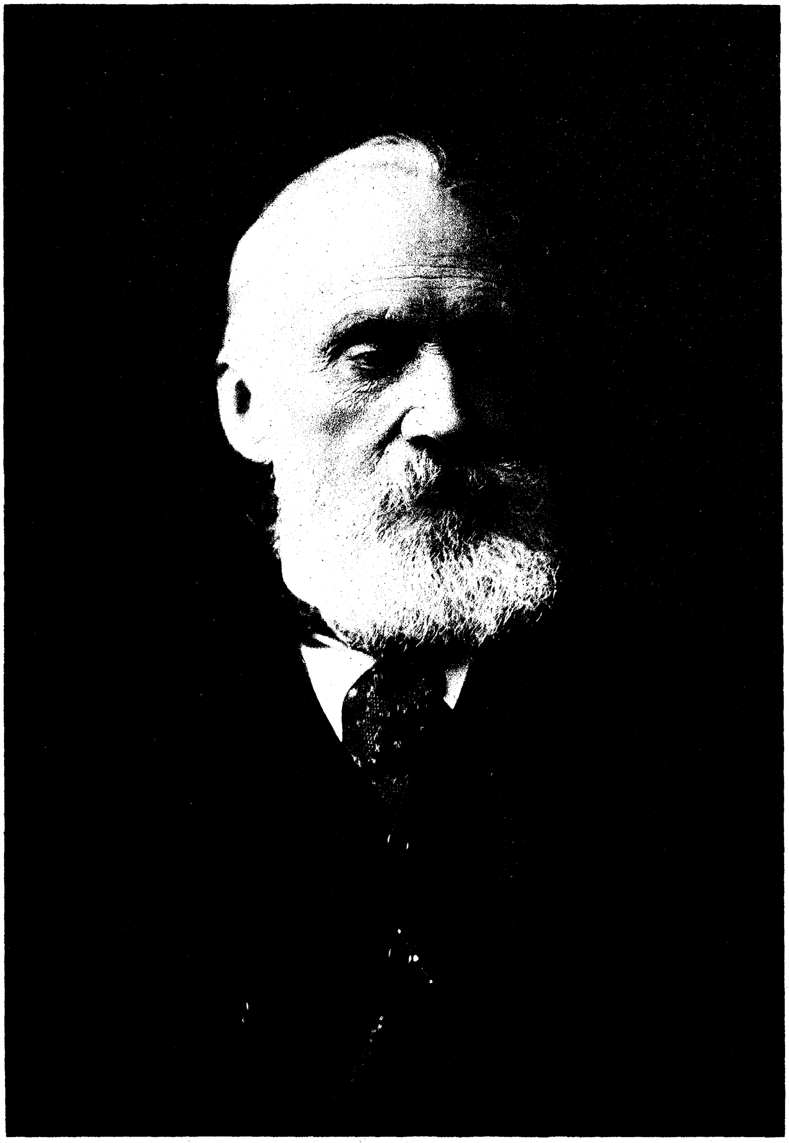
## WILLIAM THOMSON, LORD KELVIN.

Born in Belfast, June 26, 1824.
Entered University of Glasgow, 1834.
Entered at Peterhouse, Cambridge, October, 1841.
B.A. and Fellow of Peterhouse, 1845.
Professor of Natural Philosophy at Glasgow, 1846.
Fellow of the Royal Society, 1851 ; Royal Medallist, 1856 ; Copley Medallist, 1883.
Marriage to Miss Margaret Crum (deceased 1870), 1852.
First Atlantic Cables laid, 1857–58.
Permanent Atlantic Cables laid, 1865–66.
Created a Knight, 1866 ; G.C.V.O., 1896.
University of Glasgow removed to Gilmore Hill, 1870.
President of the British Association, Edinburgh, 1871.
Re-elected Fellow of Peterhouse, 1872.
Marriage to Miss Frances Anna Blandy, 1874.
Elected Foreign Associate Member of the Institute of France, 1877.
Created Knight of the Prussian Order *Pour le Mérite*, 1884.
Baltimore Lectures, 1884 ; enlarged edition, 1904.
President of the Royal Society of Edinburgh, 1886–9 ; 1895–1907.
Grand Officer of the Legion of Honour of France, 1890.
Created a Peer of Great Britain, as Baron Kelvin of Largs, 1892.
Jubilee Celebration, Glasgow, 1896.
Retired from Glasgow Professorship, 1899.
Order of Merit, original member, 1902.
Privy Councillor of Great Britain, 1902.
Chancellor of the University of Glasgow, 1904.
Deceased December 17, buried in Westminster Abbey, December 23, 1907.

LL.D. (Cantab.), D.C.L. (Oxon.), Hon. Mem. Inst. Civ. Engin. ;—Foreign Member K. Preuss. Akad., Berlin ; K. Gesell. Wiss., Göttingen ; Soc. Ital. di Scienze, Milan ; R. Accad. d. Lincei, Rome ; K. Svenska Vetensk. Akad., Stockholm ; Kais. Akad. Wiss., Vienna ; Foreign Associate of the U.S. National Academy of Sciences, Washington ; etc., etc.



Yours very truly
Kelvin

1907

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# WILLIAM THOMSON, BARON KELVIN OF LARGS. 1824—1907.

It would be impossible in an obituary of ordinary length to convey any idea of the many-sided activity by which Lord Kelvin was continually transforming physical knowledge, through more than two generations, more especially in the earlier period before practical engineering engrossed much of his attention in importunate problems which only he could solve. It is not until one tries to arrange his scattered work into the different years and periods, that the intensity of his creative force is fully realised, and some notion is acquired of what a happy strenuous career his must have been in early days, with new discoveries and new aspects of knowledge crowding in upon him faster than he could express them to the world.

The general impression left on one's mind by a connected survey of his work is overwhelming. The instinct of his own country and of the civilised world, in assigning to him a unique place among the intellectual forces of the last century, was not mistaken. Other men have been as great in some special department of physical science: no one since Newton—hardly even Faraday, whose limitation was in a sense his strength—has exerted such a masterful influence over its whole domain. He might have been a more learned mathematician or an expert chemist; but he would then probably have been a less effective discoverer. His power lay more in the direct scrutiny of physical activity, the immediate grasp of connecting principles and relations; each subject that he tackled was transformed by direct hints and analogies brought to bear from profound contemplation of the related domains of knowledge. In the first half of his life, fundamental results arrived in such volume as often to leave behind all chance of effective development. In the midst of such accumulations he became a bad expositor; it is only by tracing his activity up and down through its fragmentary published records, and thus obtaining a consecutive view of his occupation, that a just idea of the vistas continually opening upon him may be reached. Nowhere is the supremacy of intellect more impressively illustrated. One is at times almost tempted to wish that the electric cabling of the Atlantic, his popularly best known achievement, as it was one of the most strenuous, had never been undertaken by him; nor even, perhaps, the practical settlement of electric units and instruments and methods to which it led on, thus leaving the ground largely prepared for the modern refined electric transformation of general engineering. In the absence of such pressing and absorbing distractions, what might the world not have received during the years of his prime in new discoveries and explorations among the inner processes of nature.

His scientific papers, mostly mere fragments, which overflowed from his mind, as has been said, into the nearest channel of publication, have been collected by himself up to the year 1860, in somewhat desultory manner, in four substantial volumes. In addition there are three volumes of Popular Lectures and Addresses, which are more finished products, perhaps equalled in weight and scope only by those of Helmholtz. His fertility, especially in the first dozen years from 1845 to 1856, seems to be almost without precedent. Owing to the want of systematic exposition, much of this progress was grasped only imperfectly by contemporaries, and even long afterwards; but the close attention of a few master minds, including Clerk Maxwell and in a less degree Helmholtz, and in certain respects that of the school of scientific electrical engineers that was rising into confident power under his own inspiration, made up partially for this failure. In the writings on Thermodynamics and the Theory of Available Energy, this lack of consecutive arrangement has remained until the present time a serious obstacle. In the notice* of the first two volumes of the 'Collected Papers,' which was contributed to 'Nature' in 1885 by Helmholtz, the writer was so engrossed by this interesting episode as to devote nearly the whole review to its consideration; but even he has missed recognising that Thomson's 'dissipation of energy' was in 1855 determined quantitatively just as much as Clausius' 'entropy' was in the same month of the same year, and was, moreover, even then as wide in scope (*cf. infra*), making due allowance for the almost total absence of numerically exact physico-chemical data on which to develop it, as it had again become twenty years later in Helmholtz's own hands in 1882 or in those of Willard Gibbs in 1876–8.

Probably the severest ordeal to which a mass of occasional writings, evolving an entirely new range of thought, could be subjected, is that of republication after the lapse of years. The fragmentary character of the production of Thomson's papers, in scattered Journals and Transactions, naturally suggested ideas of obscurity to the workers who had time only to skim the content of separate papers without absorbing them as a connected whole; but it will probably be granted to be a most remarkable circumstance, and irrefragable proof of sureness of construction in a subject so difficult and entangled, that the papers on Thermodynamics, which also founded the modern general Theory of Energy, were capable of being reprinted in full with but slight occasional erasures, and those mainly of unessential character. Here one is, of course, leaving out of account the preliminary struggle to reconcile the apparently conflicting principles of Carnot and Joule, which forms one of the most instructive and fascinating episodes in scientific history.

We may be permitted to surmise that it was in the keen insight of these early years that his mental habitudes became fixed. His most striking characteristic all through life was insatiable thirst for knowledge, unwearied inquiry and investigation at all times, in season and out of season, combined with sympathetic interest and charming deference and encouragement to any person, however junior, who was honestly bent on the same pursuits. It is not surprising that, with new and profound views breaking in upon him from all sides, it should have grown into settled permanent habit that no mode of occupation of his time was to be allowed to interfere with the claims of scientific investigation.

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* 'Nature,' vol. 32 (1885), pp. 25–7; Helmholtz's 'Papers,' vol. 3, p. 593.

Already when he took his degree at Cambridge in the Mathematical Tripos in January 1845, it appears that many subjects closely connected with fundamental advances of the ensuing time were fermenting in his mind. It was only a few months afterwards that he at length, after years of search, discovered for the scientific world Green's 'Essay on Electricity' of 1828, ever since one of the classics of mathematical physics; he obtained, in fact by accident, a copy from his previous mathematical tutor W. Hopkins, when he recognised how much of it he had anticipated by his own more intuitive results when still a boy. Soon afterwards he went to Paris to learn physical manipulation in the laboratory of Regnault—a fact which seems to have been forgotten when he recalled in graceful terms his obligations to the French science of his youth, in an address in connexion with the celebration of the centenary of the Institute of France, of which the echoes vibrated through Paris. He has put on record that, already even at that time, he went about among the Paris booksellers, inquiring for a copy of another work of genius, which he was himself to enroll among the few supreme classics of scientific knowledge, Sadi Carnot's small tract of 1824, 'Réflexions sur la Puissance Motrice du Feu;' he found in 1845 that it was quite forgotten, though they knew in the book-shops of the social and political writings of his brother, Hippolyte Carnot, ultimately his editor and biographer (1878) in later years.

William Thomson was eight years of age at the time of his father's transference to Glasgow from Belfast, in 1832, as Professor of Mathematics. Two years later he matriculated in that University, along with his elder brother James, at the age of ten, which was young even for the Scotland of that period; and recollections have survived of the eager part taken in his father's class by the small alert figure hardly out of childish costume. The date appended to his earliest scientific paper is Frankfort, July 1840, a year before he went, at the age of seventeen, as a student to Peterhouse, Cambridge, then, as since, a college with close Scottish connexions. It is stated that during the fortnight's visit to Germany, of which a record is thus attached to the paper, he read Fourier's Treatise on Heat, of 1822, with results that are conspicuous in this and in his other earlier papers which will presently be described. To the end of his life the work of Fourier, which for the first time rendered masses of rough observational data amenable to the resources of analysis, remained for him one of the classics of mathematical literature.

The period of his undergraduate career at Cambridge, extending from October 1841, to January 1845, when he graduated as second wrangler in the Mathematical Tripos but obtained the first of the Smith's Prizes, overflowed with original mathematical activity. The sketch given below of the notes and papers which he contributed to the 'Cambridge Mathematical Journal' during this time will show how high his thoughts were removed from the didactic discipline which occupied, of necessity, the attention of the ordinary undergraduate; the main features of his subsequent mathematical interrogation of nature, the resolve not to lose himself under trains of symbolic calculations, but to draw out his analysis step by step in steady parallel with the ideas arising from direct interpretation of phenomena, are already conspicuous. Much of this habit of mind he must have taken with him from Glasgow, as the following sketch of the career of his father, a remarkable one on its own account, will perhaps show.

James Thomson the elder (1786–1849) supplies one of many cases that suggest problems as to the nature of the tendencies and faculties by which mathematicians are formed, often with very few apparent opportunities for development. He was fourth son* of James Thomson, a farmer at Annaghmore, near Ballynahinch, a village in Co. Down, once of local repute as a health-resort or spa (the house of his birth was in 1898 known as Spamount), by his wife Agnes Nesbit. His early teaching was received solely from his father. Observing his bent for scientific pursuits, of which a re-invention of the principle of dialling is quoted as an example, his father sent him to an adjacent school at Ballykine, kept by Samuel Edgar, whose son attained eminence in the Irish Presbyterian Church. Here he soon rose to be an assistant. Wishing to become a minister in the Presbyterian Church, he entered Glasgow University in 1810, at the age of thirty-four, where he studied for several sessions, supporting himself by teaching at the school in the summer. In 1814, two years after graduating M.A. at Glasgow, he was appointed headmaster of the school of 'arithmetic, book-keeping, and geography' in the Academical Institution, Belfast, newly established by public contributions in what was then a small provincial town, yet very active both intellectually and politically; in 1815 he became Professor of Mathematics in its collegiate department.

He married, in 1817, Margaret (died 1830), eldest daughter of William Gardiner, of Glasgow, and had a family of four sons and three daughters, whose education he conducted with the utmost care; of the sons the eldest were James (1822–92) and William (1824–1907).

He published numerous text-books which were deservedly very successful :—'Arithmetic,' 1819, which, having been adopted by the Irish Education Department, has passed through nearly a hundred editions; 'Trigonometry, Plane and Spherical,' 1820; 'Modern Geography,' 1827; 'Differential and Integral Calculus,' 1831; 'Euclid,' with Appendix of Pure Geometry, 1834; 'Algebra,' 1844. On turning over the pages of some of these books again, the opinion is confirmed that in elegance and conciseness and choice of material, and knowledge of the classical mathematicians, they stand quite in the front rank of the text-books of that or any period. The University relations of their author seem, however, to have been solely with Glasgow, without direct contact either with Cambridge, where modern mathematical methods had been introduced into the time-honoured curriculum by Herschel, Peacock, and Babbage about 1820, or with Dublin, which was always under French influences, and where a great native school was about to arise.

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* These facts are taken largely from an article signed T. H. in the 'Dictionary of National Biography.'

It is said that Lord Kelvin's elder brother, Professor James Thomson, undertook about 1890 a fundamental revision of the book on Arithmetic; but with characteristic passion for exactness, he became so involved in the preliminary epistemological ideas regarding the definition of number and magnitude, which more recently have amassed a large and abstruse literature of their own, that, to avoid a breakdown in health, the book had ultimately to be taken away from him by his brother and printed off.

The earliest of W. Thomson's contributions to the 'Cambridge Mathematical Journal' has two dates appended: Frankfort, July 1840, and Glasgow, April 1841. It is a defence of the accuracy of various expansions in trigonometrical series given in Fourier's 'Traité de la Chaleur,' which had been misunderstood by prominent writers. Another note on Fourier's work was published at the same time. Then followed an article "On the Linear Flow of Heat," Part I, November 1842, in which, discussing Fourier's solutions by definite integrals, he hits upon the observation that a given distribution of temperature does not in general represent a state of affairs that can have arisen from antecedents going back continuously into all past time; and in Part II, which followed in February 1843, he essays to answer the problem of the age of an assigned thermal distribution. He refers in the Reprint in 1881 to a paper published a year later for a clearer and more suitable discussion of this problem, which is, in fact, the procedure now natural for determining to what past time the convergency of the Fourier series subsists. Here, again recurring to his analogy with attractions (*infra*), he cannot resist appending a surmise that two attracting systems could not have the same field of force in a region outside them, which he is able to correct a year later still as a corollary to his famous discovery of the analytical method of image-systems.

At the same period he attended also to the now well-known theory of curvilinear isothermal co-ordinates, developed some years earlier by Lamé. His first note on this subject (May 1843) leads up, for two dimensions of space, to results now included under the applications of conjugate functions of a complex variable; and, after his custom, he drops a suggestion as to the considerable probability in favour of an extension of the theory to three dimensions. This is soon followed by a paper in which he investigates that question and withdraws his surmise; in it, by the Fourier method of flux, applied to a curvilinear element of volume, he leads directly to the characteristic equation of flow of heat in general co-ordinates, and thus replaces Lamé's own laborious analytical transformation.* Here he is led to the stream function for flux symmetrical around an axis, in which, as he remarks in the Reprint, he had been anticipated by Stokes in 1842. He returns to the original problem in November 1844, in connexion with a recent memoir by Lamé, which had given a complete solution, and also a note by Bertrand. A concise independent demonstration of Dupin's famous cognate theorem, that a triple system of mutually orthogonal surfaces intersect along their lines of curvature, forms the subject of another note.

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* Jacobi's procedure for the same purpose by the Calculus of Variations is of date 1847, in his memoir on Laplace's Equation in 'Crelle,' vol. 37, or 'Werke,' vol. 2, pp. 191–216.

About the same time he was already paying attention to a subject which, in its geodetic application, absorbed him much in later years, the theory of the steady configurations of revolving masses of homogeneous fluid. In November 1844, he publishes a proof of the result—obtained by the brief, direct, geometrical mode of argument in which he always delighted—that, provided the free surface of the fluid be an ellipsoid, whether it be one of Maclaurin's pair or that of Jacobi, the force of gravity must vary inversely as the distance of the tangent plane from the centre of the surface.

At the very time of his Mathematical Tripos and Smith's Prize Examinations, at Cambridge, he was preparing for press extensive papers on the Reduction of the General Equation of Surfaces of the Second Order and on the Lines of Curvature of such surfaces (Reprint, pp. 55–71).

It is hardly matter of wonder that the result of all this scientific activity of the highest order was that, in the Mathematical Tripos at the beginning of 1845, he only attained to the second place in the list. Two, at any rate, of the four examiners were men of mark, Robert Leslie Ellis and Harvey Goodwin. Inspection of the papers set by them, which were, on the whole, equal to their reputation, does not lend probability to the tale that some theorems taken from Thomson's published work were among the questions proposed, which, however, their author found himself unable to answer, though his rival (Stephen Parkinson, afterwards D.D., F.R.S., and Tutor of St. John's College) did not allow them to escape him.* That the order of the result did not arise in any way from lack of appreciation is in accordance with the contemporary statement, that the examiners had given it out that they did not consider themselves worthy even to mend Thomson's pens. In the award of the Smith's Prizes immediately following, made by the mathematical professors under less restricted conditions, there could thus have been no room for so equivocal a result.

We now return to September 1841, when, at the age of seventeen, a month before he entered at Peterhouse, he sent to the 'Cambridge Mathematical Journal' a paper "On the Uniform Motion of Heat in Homogeneous Solid Bodies, and its connexion with the Mathematical Theory of Electricity," doubtless another result of his study of Fourier's treatise mentioned above. By the time it was published, under the signature P. Q. R., in February 1842, he was able to prefix a note stating that, in the mathematical theorems reached, he had been largely anticipated by the great French mathematician Chasles. A further note to the reprint in 'Phil. Mag.,' 1854, relates the history of one of his great discoveries, this time a personal one. He there adds to his anticipators the name of Gauss, whose treatment of the subject had 'appeared shortly after Chasles' enunciations: and after all he found that these theorems had been discovered and published in the most complete and general manner, with rich applications to the theories of electricity and magnetism, more than ten years previously, by Green. It was not until early in 1845 that the author, after having inquired for it in vain for several years, in consequence of an obscure allusion to it in one of Murphy's papers, was fortunate enough to meet with a copy of the remarkable paper ('An Essay on the Application of Mathematical Analysis to the Theories of Electricity and Magnetism,' by George Green, Nottingham, 1828), in which this great advance in physical mathematics was first made. It is worth remarking that, referring to Green as the originator of the term, Murphy gives a mistaken definition of 'potential.' It appears highly probable that he may never have had access to Green's 'Essay' at all, and that this is the explanation of the fact (of which any other explanation is scarcely conceivable), that in his treatise on electricity (Murphy's 'Electricity,' Cambridge, 1833), he makes no allusion whatever to Green's discoveries, and gives a theory in no respect pushed beyond what had been done by Poisson. All the general theorems on attraction which Green, and the other writers referred to, demonstrated by various purely mathematical processes, are seen as axiomatic truths in approaching the subject by the way laid down in the paper which is now republished. The analogy with the conduction of heat, on which these views are founded, has not, so far as the author is aware, been noticed by any other writer."

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* Another form of the tale is that in the Smith's Prize Examination two of the candidates answered a question in such striking and identical terms that investigation was made; when it turned out that the answers were taken from Thomson's path-breaking paper of four years previously, next to be referred to, which had appeared under his customary signature P. Q. R. As a fact, Earnshaw did set a question asking for a development of the general analogy between the theory of attractions and the conduction of heat.

Professor S. P. Thompson relates that, in answer to a question, Lord Kelvin recently told him that he deserved his defeat, owing to 'bad generalship' in spending too much time over problems that would not come out.

The analysis in the part of this very remarkable paper in which he had been anticipated by Chasles and Gauss, retains its place almost unaltered in the text-books, to this day, as the classical and most compact method of treating such subjects as the attraction of ellipsoids and ellipsoidal shells;* but more remarkable from a youth at the age of seventeen is the analogy, above referred to, between electric force and thermal flux, fundamentally illuminating to both, and pregnant with the great advances then impending in physical science.

The story of Thomson's discovery of George Green may now be completed from a footnote in the paper of 1845. "I should add that it was not till the beginning of the present year (1845) that I succeeded in meeting with

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* *Cf.*, for instance, Thomson and Tait's 'Nat. Phil.'

Green's Essay. The allusion made to his name with reference to the word 'potential' ('Mathematical Journal,' vol. iii, p. 190) was taken from a memoir of Murphy's, 'On definite Integrals with Physical Applications,' in the 'Cambridge Transactions,' where a mistaken definition of that term as used by Green is given." In 1850 he sent Green's 'Essay' to be reprinted in 'Crelle's Journal,' vol. 39, with a prefatory notice, and it thenceforth assumed its place on the Continent as a classic: recently, in reply to inquiries about Green's biography, he wondered why he had not reprinted it in his own 'Cambridge Journal.'

Early scientific impressions seem to have persisted with Thomson throughout life. To the end the names of Fourier and Green, whose fundamental importance he had been instrumental in elucidating in his own youthful work, remained for him among the very greatest in the scientific firmament.

We now pass on again to 1845; at the beginning of this year he had taken his degree, and then appeared before the public in his own name as the Editor of the 'Journal.' He planned a series of papers "On the Mathematical Theory of Electricity in Equilibrium," the first of which appeared in November 1845, "On the Elementary Laws of Statical Electricity." The paper had been published in an earlier form in French in Liouville's 'Journal de Mathématiques' about the middle of the year. In the course of it he already records incidentally the solution of the problem of the mutual influence of two charged spheres by his method of successive point-images. Yet the most interesting part of it is the end, where he applies himself to the elucidation of Faraday's physical views on electric induction. He points out that Faraday's idea of flux of induction involves precisely the analogy of electric force with flux of heat which he had himself developed in his earliest paper of 1841, the flow of heat being obviously conditioned, in accordance with Faraday's phrase, by the action of contiguous particles. He now remarks that Faraday's idea of polarisation of the particles of the dielectric medium is the exact analogue of Poisson's theory of induced magnetism, and (in the English edition of the paper) by reasoning on Poisson's principles he obtains the explanation of dielectric influence which holds good unchanged in electron-theory to this day;* nor has it ever been better or more succinctly expressed. Curiously, though he expressly states that the effect becomes smaller as the molecules are fewer, yet his words seem to imply acceptance of the failure of Faraday's efforts with gases as evidence that the free molecules of a gaseous dielectric are not electrically polarisable. In a note to the 'Phil. Mag.' Reprint of 1854, Thomson points out that this analogy of conductivity for heat is the precise equivalent of Faraday's "conducting power of a medium for lines of force"—a point of view which, however, was not reached by Faraday until five years later ('Exp. Res.,' Series 27, October 1850) as the expression of his experimental synthesis of the relations and properties of lines of magnetic force in iron and other highly magnetic media. Earlier, in 'Exp. Res.,' Series 11, of date November 1837, where the idea of curved lines of electrostatic induction is reached, it was the conception of tension along the lines and sideway pressure that had guided Faraday's thought.

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* The same theory of molecular polarisation was developed independently by Mossotti in the year following, 'Mem. della Soc. Italiana,' vol. 24, as Thomson subsequently remarks; it is often connected with his name.

The train of mathematical development of the ideas of Faraday, which was subsequently in Maxwell's hands to be moulded into our present theory of the phenomena of electricity and radiation, was begun in a short note which appeared early in 1847.* Referring to the concluding paragraphs of the Eleventh Series of Faraday's 'Researches' (November 1837), with their dominant idea of induction along curved lines of force,† supposed to be transmitted essentially through interaction of contiguous particles, he states that this theory of Electrostatical Induction "suggests the idea that there may be a problem in the theory of elastic solids corresponding to every problem connected with the distribution of electricity on conductors, or with the forces of attraction and repulsion exercised by electrified bodies. The clue to a similar representation of magnetic and galvanic forces is afforded by Mr. Faraday's recent discovery (November 1845) of the affection with reference to polarised light of transparent solids subjected to magnetic or electromagnetic forces." Referring to Stokes' classical analysis of the Equilibrium of Elastic Bodies which had recently been published (1845), he points out that the states of strain that can persist freely in the interior of homogeneous elastic matter, under the appropriate surface forces and no internal ones, are those in which the displacement $(\alpha, \beta, \gamma)$ satisfies the relation that $\nabla^2\alpha \,.\, \delta x + \nabla^2\beta \,.\, \delta y + \nabla^2\gamma \,.\, \delta z$ is a perfect differential, say $\delta\phi$. He then restricts the discussion to the case of a medium incapable of compression.

In the special case $\alpha\delta x + \beta\delta y + \gamma\delta z = -\delta r^{-1}$ of this relation, $(\alpha, \beta, \gamma)$ coincides with the electric force due to unit charge at the origin, here represented (*e.g.*) by a small vesicle in the medium containing gas which exerts pressure in all directions.

In the next special case

$$\left(\frac{\partial\beta}{\partial z} - \frac{\partial\gamma}{\partial y}\right)\delta x + \left(\frac{\partial\gamma}{\partial x} - \frac{\partial\alpha}{\partial z}\right)\delta y + \left(\frac{\partial\alpha}{\partial y} - \frac{\partial\beta}{\partial x}\right)\delta z = -\delta\frac{lx + my + nz}{r^3},$$

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* 'Cambridge and Dublin Math. Journal,' vol. 2; 'Math. and Phys. Papers,' vol. 1, pp. 76–80; dated from Glasgow College, November 28, 1846.

† There will doubtless always be difference of opinion regarding the scope and definiteness of Faraday's idea of lines of force, which he used so effectively as a basis of geometrical reasoning about physical forces. That the elastic interaction here enunciated is not inconsistent with Faraday's own view appears from the following extract ('Exp. Res.,' No. 1304):—"I have used the phrases *lines of inductive force* and *curved lines* of force in a general sense only, just as we speak of the lines of magnetic force. The lines are imaginary, and the force in any part of them is the resultant of compound forces, every molecule being related to every other molecule in *all* directions by the tension and reaction of those which are contiguous." In his later magnetic work (1850) the language, however, suggests that the lines are to him more than mathematical representations.

the vector occurring on the left thus represents the *magnetic* force of a magnetic bipole situated at the origin and lying in the direction $(l, m, n)$; it is expressed as the curl of the previous electric force $(\alpha, \beta, \gamma)$, that is, as twice the differential rotational displacement at the point considered.

Finally, if $\alpha = -\frac{l}{r}+\frac{1}{2}\frac{\partial}{\partial x}\frac{lx+my+nz}{r}$, with the related expressions for $\beta$ and $\gamma$, the curl of $(\alpha, \beta, \gamma)$ now represents the magnetic force due to a unit current-element situated at the origin along the direction $(l, m, n)$.

In these statements it is implied (already in 1847) that magnetic force is related to electric force as involving the differential rotation or curl of the latter. If he had probed the matter only a little further, he would have been forced to recognise, on Faraday's principles, that it is the time-gradient of the magnetic force that is so related; and the Maxwellian theory of the æther might have opened up to his view. But he winds up the brief and hurried note characteristically as follows:—"I should exceed my present limits were I to enter into a special examination of the states of a solid body representing various problems in electricity, magnetism, and galvanism, which must therefore be reserved for a future paper. *Glasgow College,* November 28, 1846." The future paper seems never to have arrived, but the present one was enough to give a lead to Maxwell's earliest studies.*

It was in the previous year, November 1845, that Faraday communicated to the Royal Society what Lord Rayleigh has described as one of the finest of his discoveries, the detection of a relation connecting magnetism and light, in the circumstance that the plane of polarisation of light passing through matter is rotated by a magnetic field. One may safely assume that this result must have been deeply pondered over by Thomson: we are justified by the quotation, *supra,* in ascribing to its influence the idea underlying and pervading this note, that if electric force is represented by displacement of the particles of a medium the magnetic force is related to the resulting rotation (in Stokes' sense) of the differential element of volume.

After this great refusal to proceed, the subject of the underlying mechanism of electromagnetic phenomena goes out of sight for eleven years, until 1856.

This is a convenient place to refer to a note, of the end of the same year,† extending his solution (iii) above to elastic solids not incompressible: a displacement $(\alpha, \beta, \gamma)$ of type given by $\alpha = -g\frac{l}{r}+\frac{1}{2}f\frac{\partial}{\partial x}\frac{lx+my+nz}{r}$ represents the result of a force applied to an infinitely extended solid in the direction $(l, m, n)$ at the origin of co-ordinates. He points out that general solutions may be developed by combining such "sources of strain" as he had combined sources of heat in his earlier paper,—a method succinctly and exhaustively exemplified by Stokes two years later in his memoir "On the Dynamical Theory of Diffraction," and in fact utilised by Kirchhoff, Hertz, and all subsequent analysts in all domains of elastic theory.

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* See in fact the reference to it in his first memoir "On Faraday's Lines of Force" (1856), in Maxwell's 'Scientific Papers,' vol. 1, p. 188; and repeatedly in the mechanical theory in "Physical Lines of Force," 'Phil. Mag.' 1861–2. The future paper did arrive in 1889, having been then written for 'Math. and Phys. Papers,' vol. 3; *cf. infra,* p. lxvii.

† 'Cambridge and Dublin Math. Journal,' vol. 3, p. 87.

Hitherto we have recounted work on the more physical side of electrical theory. Thomson's early home training in Euclidean geometry shone conspicuously in his investigations on the distribution of electricity on spherical conductors. The beautiful and now familiar idea of electrical images was first published in Liouville's 'Journal des Mathématiques,' in extracts from three letters to the editor, in October 1845, and in June 1846. The first of these extracts* recalls conversations with Liouville on this subject in Paris, in 1845, whither Thomson had gone soon after taking his degree, in order to gain physical experience in Regnault's laboratory. The second letter consists mainly of an exposition of the system of orthogonal co-ordinates (*cf. supra*, p. vii) in space, which is suitable for treating the problem of two spheres. A third letter of September 1846, states the result for the distribution of electricity on a thin spherical bowl: the analysis which led to it did not see the light until 1869, when it was published, with full numerical tables to illustrate electrostatic shielding, in 'Electrostatics and Magnetism,' pp. 178–191. The problem of the mutual influence of two electrified spheres was treated in full by the method of successive images, with numerical results calculated for a projected absolute electrometer on this principle, in 'Phil. Mag.,' 1853, and 'Electrostatics and Magnetism,' pp. 87–97. This method had been described to the British Association at Cambridge as early as 1845. The results relating to the mutual attraction of two equal spheres had been published (*supra*, p. x) in 'Cambridge and Dublin Math. Journal' the same year: it appears that the main parts of the investigation were communicated to Liouville by letter in 1849. Already in 1845 he deduced the force of attraction from the energy of the distribution, or "mechanical value," as he called this function whose minimum property had attracted his attention as utilised in Gauss' memoir of 1839; the function appeared for the first time as energy in Helmholtz's 'Erhaltung der Kraft', in 1847, as he learned later.

His instant absorption of the contents of Green's 'Essay,' and the rapid expansion of the method of images, are jointly illustrated by a note of October 1845 (when he was back in Peterhouse from Paris), in the 'Cambridge and Dublin Math. Journal,' in which he briefly indicates the application of this method to the determination of the induced magnetisation in a plate of soft iron, a problem of which the solution had been one of Green's analytical *tours de force*. As is now well known, the problem for an infinite mass of iron with a plane face is solved by a single image; and he passes from this to a solution for a plate, which he proves to be identical with Green's, by successive reflexions.

In Liouville's 'Journal,' in 1847, reprinted in part in 'Cambridge and Dublin

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* Reprint, 'Electrostatics and Magnetism,' p. 144.

Math. Journal' early in 1848, he essays to demonstrate both the determinacy and the existence of the solution of the general differential equation of flux, thermal or electric, subject to appropriate conditions over a boundary. The method, reprinted in Thomson and Tait's 'Nat. Phil.,' is to invent a volume integral of an essentially positive function, for which the characteristic equation of the flux is the condition of a stationary value as determined by the calculus of variations. As regards proof of the existence of a solution satisfying the conditions, this argument, employed also by Riemann in 1851, afterwards gave way, so far as the requirements of abstract analysis are concerned, and became discredited generally, through the destructive criticism of Weierstrass. It has doubtless been gratifying to believers in the policy of encouraging physical intuition as a mathematical resource, to learn that a recent improvement by Hilbert seems to have removed the difficulties.

Various less important analytical contributions to the 'Cambridge and Dublin Math. Journal' need not be mentioned. The papers that have now been passed under review occupy 191 pages in the reprint on 'Electrostatics and Magnetism,' and the first 112 pages of vol. 1 of the 'Mathematical and Physical Papers.'

The activity of Thomson in connexion with the early British mathematical journals seems to demand special record. It has been seen that, beginning at seventeen years of age, he was an active supporter during his undergraduate days of the original 'Cambridge Math. Journal.' Whatever was the cause, his contributions were all anonymous, being usually signed 'P. Q. R.,' but in one case 'N. N.' After he took his degree this journal gave place to the 'Cambridge and Dublin. Math. Journal,' and his first paper, of date October 14, 1845, appears under his name as "W. Thomson, B.A., Fellow of St. Peter's College," while the title-page of the first volume, of date 1846, gives his name as sole editor, with an additional description as Professor of Natural Philosophy in the University of Glasgow. The last volume of the previous series had appeared with the name of R. L. Ellis as editor. Thomson's connexion with the 'Journal' continued until vol. 8, 1853; in this volume he was associated with N. M. Ferrers; the last volume, 9, was brought out, in 1853, by Ferrers alone. In 1857 the 'Quarterly Journal of Math.' began its course, in order, as the editors expressed it, to enable English mathematicians to take their part in the rapid circulation and interchange of ideas. Its activity, which still continues, was then under the direction of J. J. Sylvester and N. M. Ferrers, assisted by G. G. Stokes, A. Cayley, and C. Hermite. Though the physical interest subsequently waned, the first number contains (pp. 57–77) a paper by W. Thomson, dated March 10, 1855, of fundamental importance in the then nascent general theory of energy, as will appear later.

The fact that the carrying on of the 'Journal' for the eight years above described, with the editor so far from Cambridge as Glasgow, was a troublesome task, appears frequently in Thomson's correspondence with Stokes, in which he is often urging the necessity of getting material to enable an impending part of the 'Journal' to be completed. The two friends exerted themselves to keep it going, and the 'Journal' has ever since retained its position as a classic, largely through their contributions in the domain of Mathematical Physics. In particular they planned a joint series of articles entitled "Notes on Hydrodynamics," designed for didactic purposes, which assisted conspicuously towards crystallising that subject into a formal science. The editor also pressed into the service his Scottish physical friends, such as Macquorn Rankine, and his brother James Thomson, who both made contributions of fundamental value—Rankine expounding the general theory of elasticity, including MacCullagh's rotational elasticity of the æther which he was the first to formulate in an objective way, while James Thomson treated, in most original manner, of the laws of spiral springs, and the influence of internal strain of the strength of materials, as well as the lowering of the freezing-point by pressure; and Dublin, in the persons of W. R. Hamilton, G. Salmon, S. Haughton, R. Townsend, and others, contributed her share.

The last of the notes on hydrodynamics, of February 1849,* contains his theorem that the motion of a fluid mass, arising from given movement impressed on its boundary, is the one involving the least possible energy: a proposition which, when extended in 1863 to any dynamical system† whatever, set in motion impulsively by imparting specified velocities, became fundamental in general dynamics under the name of Thomson's theorem.

As regards the contributions of Thomson and his friends, the 'Journal' fulfilled in the highest degree the main function of such a publication; everything was concise and pointed, and adapted to excite the interest of readers who were not specialists: there were few monographs. The reference in the admirable prefatory remarks introducing its successor, the 'Quarterly Journal,' and expressing the editors' sense of the heavy duties and responsibilities which it imposed, can easily be appreciated. "All who are interested in the cause of Mathematical Science are aware of the great and beneficial influence which has been brought to bear upon the study of Mathematics in this country by the publication of the 'Cambridge,' and subsequently of the 'Cambridge and Dublin Mathematical Journals,' which, if they cannot strictly be said to have created the present school of English mathematicians, may fairly claim to have provided the arena in which they have been able to measure their strength and give evidence of their capabilities. Causes upon the nature of which it is not necessary here to insist, having recently led to the discontinuance of the latter of these Journals, it became a question and a subject of anxiety among several of its former contributors and supporters how its place was to be supplied."

In November 1845, Faraday was able to communicate to the Royal Society the successful issue of long-continued efforts to find a connexion between light and electricity. This was the brilliant detection of the rotation of the plane of polarisation of light passing through material bodies by a magnetic field, which formed the beginning of a fresh series of discoveries relating to the magnetic state in all kinds of matter; and he pointed out its distinction from the intrinsic rotation produced (*e.g.*) by passage through quartz or sugar solution.

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* 'Math. and Phys. Papers,' vol. 1, p. 107.

† 'Thomson and Tait,' § 312.

Thomson, in May 1856,* probed the underlying dynamical meaning of this difference. The result is emphasised, that the existence of structural rotation is inconsistent with complete homogeneity of the transmitting medium, and points to its containing molecular elements, which must be of essentially spiral quality, due either to spiral crystalline arrangement of non-spiral molecules as in quartz, or to molecules each structurally spiral as in active liquids. The introductory paragraphs, which were fateful in the history of electrical science, are here quoted. Maxwell, in his 'Treatise' (1872, § 831), reproduces the second paragraph as an "exceedingly important remark" or argument on which the whole subject of the relation of light to magnetism must be based.

"The elastic reaction of a homogeneously strained solid has a character essentially devoid of all helicoidal and of all dipolar asymmetry. Hence the rotation of the plane of polarisation of light passing through bodies which either intrinsically possess the helicoidal property (syrup, oil of turpentine, quartz crystals, etc.), or have the magnetic property induced in them, must be due to elastic reactions dependent on the heterogeneousness of the strain through the space of a wave, or to some heterogeneousness of the luminous motions† dependent on a heterogeneousness of parts of the matter of lineal dimensions not infinitely small in comparison with the wave-length. An infinitely homogeneous solid could not possess either of those properties if the stress at any point of it was influenced only by parts of the body touching it; but if the stress at one point is directly influenced by the strain in parts at distances from it finite in comparison with the wave-length, the helicoidal property might exist, and the rotation of the plane of polarisation, such as is observed in many liquids and in quartz crystals, could be explained as a direct dynamical consequence of the statical elastic reaction called into play by such a strain as exists in a wave of polarised light. It may, however, be considered more probable that the matter of transparent bodies is really heterogeneous from one part to another of lineal dimensions not infinitely small in comparison with a wave-length, than that it is infinitely homogeneous and has the property of exerting finite direct 'molecular' force at distances comparable with the wave-length, and it is certain that any spiral heterogeneousness of a vibrating medium must, if either right-handed or left-handed spirals predominate, cause a finite rotation of the plane of polarisation of all waves of which lengths are not infinitely great multiples of the steps of the structural spirals. Thus a liquid filled homogeneously with spiral fibres, or a solid with spiral passages through it of steps not less than the forty-millionth of an inch, or a crystal with a right-handed or a left-handed geometrical arrangement of parts of some such lineal dimensions as the forty-millionth of an inch, might be certainly expected to cause either a right-handed or a left-handed rotation of ordinary light (the wave-length being 1/40000 of an inch for homogeneous yellow).

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* 'Roy. Soc. Proc.,' vol. 8, pp. 150–8 ; 'Baltimore Lectures,' Appendix F.

† "As would be were there different sets of vibrating particles, or were Rankine's important hypothesis true, that the vibrations of luminiferous particles are directly affected by pressure of a surrounding medium in virtue of its inertia."

"But the magnetic influence on light discovered by Faraday depends on the direction of motion of moving particles. For instance, in a medium possessing it, particles in a straight line parallel to the lines of magnetic force, displaced to a helix round this line as axis, and then projected tangentially with such velocities as to describe circles, will have different velocities according as their motions are round in one direction (the same as the nominal direction of the galvanic current in the magnetising coil), or in the contrary direction. But the elastic reaction of the medium must be the same for the same displacements, whatever be the velocities and direction of the particles; that is to say, the forces which are balanced by centrifugal force of the circular motions are equal, while the luminiferous motions are unequal. The absolute circular motions being, therefore, either equal or such as to transmit equal centrifugal forces to the particles initially considered, it follows that the luminiferous motions are only components of the whole motion, and that a less luminiferous component in one direction, compounded with a motion existing in the medium when transmitting no light, gives an equal resultant to that of a greater luminiferous motion in the contrary direction compounded with the same non-luminous motion. I think it is not only impossible to conceive any other than this dynamical explanation of the fact that circularly polarised light transmitted through magnetised glass parallel to the lines of magnetising force, with the same quality, right-handed always, or left-handed always, is propagated at different rates according as its course is in the direction or is contrary to the direction in which a north magnetic pole is drawn, but I believe it can be demonstrated that no other explanation of that fact is possible. Hence it appears that Faraday's optical discovery affords a demonstration of the reality of Ampère's explanation of the ultimate nature of magnetism, and gives a definition of magnetisation in the dynamical theory of heat. The introduction of the principle of moments of momenta ('the conservation of areas') into the mechanical treatment of Mr. Rankine's hypothesis of 'molecular vortices,' appears to indicate a line perpendicular to the plane of resultant rotatory momentum ('the invariable plane') of the thermal motions as the magnetic axis of a magnetised body, and suggests the resultant moment of momenta of these motions as the definite measure of the 'magnetic moment.' The explanation of all phenomena of electromagnetic attraction or repulsion, and of electromagnetic induction, is to be looked for simply in the inertia and pressure of the matter of which the motions constitute heat. Whether this matter is or is not electricity, whether it is a continuous fluid interpermeating the spaces between molecular nuclei, or is itself molecularly grouped, or whether all matter is continuous, and molecular heterogeneousness consists in finite vortical or other relative motions of contiguous parts of a body, it is impossible to decide, and, perhaps, in vain to speculate, in the present state of science.

"I append the solution of a dynamical problem for the sake of the illustrations it suggests for the two kinds of effect on the plane of polarisation referred to above."

After some dynamical gyrostatic effects of cognate character have been discussed by mathematical analysis, the paper ends by a paragraph as follows:—"From these illustrations it is easy to see, in an infinite variety of ways, how to make structures, homogeneous when considered on a large enough scale, which (1) with certain rotatory motions of component parts having, in portions large enough to be sensibly homogeneous, resultant axes of momenta arranged like lines of magnetic force, *shall have the dynamical property by which the optical phenomena of transparent bodies in the magnetic field are explained;* (2) with spiral arrangements of component parts, having axes all ranged parallel to a fixed line, *shall have the axial rotatory property corresponding to that of quartz crystal;* and (3) with spiral arrangements of component groups, having axes totally unarranged, *shall have the isotropic rotatory property possessed by solutions of sugar and tartaric acid, by oil of turpentine, and many other liquids.*"

He returns incidentally to the subject,* insisting that "electrodynamic capacity" is "identical in meaning with the 'simple mass-equivalent' in the motion of Attwood's machine as ordinarily treated." This "it seems quite certain must be owing to true inertia of motions accompanying the current, chiefly rotatory, with axes coinciding with the lines of magnetic force in the iron, air, or other matter in the neighbourhood of the conductor, and continuing unchanged so long as the current is kept unchanged."

It may be recalled that it was in 1858 that the dynamical theory of vortex motion in fluids was created by the great memoir of Helmholtz ('Crelle's Journal,' vol. 55). Maxwell, in his 'Treatise,' § 822, improving upon a section in his memoir "On Physical Lines of Force," of 1862, used all the data available to connect the propagation of light with the magnetism, on the basis that the magnetic force is a vortical molecular phenomenon in the medium which combines in a scalar manner with the vortical quality in the motion that constitutes radiation, as regards the energy function which determines the dynamics, and also that the magnetic field alters by the light-motion as vortices in fluid would do. He finds that this hypothesis is the same thing, as regards continuous propagation, as a scalar connexion between the motion of the medium and the total electric current which is the equivalent of the magnetism. The more recent observations, however, connect the optical effect with the magnetisation instead of the magnetic force, as modern molecular theory would indicate.

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* 'Roy. Soc. Proc.,' vol. 11, 1861, footnotes p. 273.

At the beginning of the same year 1856, near the end of the twenty-fourth year of his age, and two years after taking his degree at Cambridge, James Clerk Maxwell communicated to the Cambridge Philosophical Society his earliest electrical memoir, "On Faraday's Lines of Force." His genius was as systematic as Thomson's was desultory. This paper, and the subsequent one "On Physical Lines of Force," in the 'Philosophical Magazine' for 1861, were as much an exposition of Thomson's interpretations as the former was also of Faraday's own views. A study of the general sketches prefixed to these papers would be, in fact, one of the best ways of appreciating the extent of Thomson's activity in electrical theory up to that date. Nothing that Maxwell wrote is more illuminating than these preliminary essays on the scope of scientific explanation, and the enforcement, from the examples of Faraday and Thomson, of the effective superiority of graphic and plastic analogical reasoning over self-centred abstract calculations. When he passes on to try to fit together the various partial aspects of the electrical theory into a single connected scheme, the lucidity of the previous general sketch deserts him, as might be expected from the difficulty of the enterprise; though the defect is possibly in part due to over-elaboration of the analytical apparatus, which could have been better grasped as a whole if more condensed. The same fault attaches in a greater degree to the treatise on 'Electricity and Magnetism,' made up as it seems of a series of partial preliminary sketches, intended to be welded ultimately into a systematic treatise, but which he appears to have been induced to throw into the press on his coming to Cambridge in 1871, in order to supply the urgent need of some accessible exposition of the new physical views for the use of students. A remark of his has been handed down which points in this direction—to the effect that the aim of his book was not to finally expound the theory for the world, but to educate himself by the presentation of a view of the stage he had reached. This would, at any rate, account for the disjointed character of the 'Treatise,' and the sudden transitions in the points of view between different chapters, which have been found to be so puzzling and have naturally induced remonstrance from readers unacquainted with the evolution of the subject. Instead of tying himself down to a definite ordered exposition, with all material which cannot be fitted into it rejected as irrelevant or unprofitable, the aim is purposely to keep the subject open, to record all the converging considerations and lines of argument that have a chance of proving useful or suggestive for the ultimate unification. Much misunderstanding has thereby been caused both in this country and abroad, and many complaints of the absence of logical cohesion; not a few reconstructions have been offered by stringing together excerpts so as to make one consecutive story, filling in the gaps according to predilection, but ignoring the remaining aspects as mere disturbances of the train of explanation. And for didactic purposes this has doubtless been a help; though we may agree with Boltzmann that it could hardly be done better than Maxwell did it himself, in the introductory expositions prefixed to his earlier papers; or, later, in an appendix to his memoir ('Phil. Trans.,' 1868) on the ratio of the electric units, where he sets forth a skeleton of the theory of light, of the approved modern heuristic pattern, in order to meet a demand for a concise conspectus of its content. It is noteworthy that Maxwell's general argument there for the existence of a transmitting medium is precisely that, without support from an æther, the forces between electric bodies cannot be arranged, on any existing theory, so as to form a balance, as regards either momentum or energy—that without it reaction does not balance action and the energy is not conserved.

In the final instalment of the paper "On Physical Lines of Force," 'Phil. Mag.,' February 1862, Maxwell emphasises Thomson's demonstration, quoted above, that magnetism involves essentially the rotational motion of something around its lines of force. He probes and developes tentatively a theory of the magnetic force as due to the centrifugal force of vortices associated with the molecules of matter, assuming that these vortices "consist of the same matter the vibrations of which constitute light." But directly afterwards* he seems almost to repent of this unnecessary restriction, under the fascination of Weber's theory of moving electric particles:—"I am inclined to believe that iron differs from other substances in the manner of its action as well as in the intensity of its magnetism; and I think this behaviour may be explained on our hypothesis of molecular vortices, by supposing that the particles of the *iron itself* are set in rotation by the tangential motion of the vortices, in an opposite direction to their own. These large heavy particles would thus be revolving exactly as we have supposed the infinitely small particles constituting electricity to revolve, but without being free like them to change their place and form currents." Knowledge has crystallised since this remarkable passage was written: the "infinitely small particles constituting electricity" have undergone a natural evolution from Weber's attracting particles into the electrons appropriate to Maxwell's theory: it is *their* rotation in the molecule that conditions the magnetic phenomena: and Maxwell's notion of a molecular aggregate in iron rotating as a whole may yet be a clue to the explanation of ferromagnetics. His analytical development, however, is difficult to interpret on either view. In the "Dynamical Theory," 'Phil. Trans.,' 1864, he harks back (§ 8) to the idea of rotation of the æther: the time was not yet ripe for the electrons as the originators of the disturbances whose propagation he studied so closely.

It seems that it was not until 1864 that Maxwell had reached the electric theory of light, these studies in the Faraday magneto-optic relation, even as reproduced in modified form in the 'Treatise,' being purely tentative and provocative of the deeper plunge. Thus, writing in 1864, "the conception of the propagation of transverse magnetic disturbances to the exclusion of normal ones is distinctly set forth by Professor Faraday in his 'Thoughts on

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* 'Scientific Papers,' vol. 1, p. 507.

Ray Vibrations,' 'Phil. Mag.,' 1864. The electromagnetic theory of light, as proposed by him, is the same in substance as that which I have begun to develope in this paper, except that in 1846 there were no data to calculate the velocity of propagation."* Maxwell did not return to the subject, but left the application to the laws of optical reflexion and dispersion to be developed by others, Helmholtz, FitzGerald, etc.; some of the reasons why he did not consider the detailed optical theory for material bodies to be quite ripe for treatment appear from his correspondence with Stokes, recently published.†

This is the suitable place to insert a summing up of his own ideas on this subject by Thomson, made just before Maxwell began his attack on the physical side of the problem of the æther. It forms the peroration to an eloquent discourse on atmospheric electricity, delivered at the Royal Institution, May 18, 1860.‡

"The speaker could not conclude without guarding himself against any imputation of having assumed the existence of two electric fluids or substances, because he had frequently spoken of the vitreous and resinous electricities. Dufay's very important discovery of two modes or qualities of electrification led his followers too readily to admit his supposition of two distinct electric fluids. Franklin, Æpinus, and Cavendish, with a hypothesis of one electric fluid, opened the way for a juster appreciation of the *unity* of nature in electric phenomena. Beccaria, with his 'electric atmospheres,' somewhat vaguely struggled to see deeper into the working of electric force, but his views found little acceptance, and scarcely suggested inquiry or even meditation. The eighteenth century made a school of science for itself, in which, for the not unnatural dogma of the earlier schoolmen, 'matter cannot act where it is not,' was substituted the most fantastic of paradoxes, *contact does not exist.* Boscovich's theory was the consummation of the eighteenth century school of physical science. This strange idea took deep root, and from it grew up a barren tree, exhausting the soil and overshadowing the whole field of molecular investigation, on which so much unavailing labour was spent by the great mathematicians of the early part of our nineteenth century.§ If Boscovich's theory no longer cumbers the ground, it is because one true philosopher required more light for tracing lines of electric force.

"Mr. Faraday's investigation of electrostatic induction influences now every department of physical speculation, and constitutes an era in science. If we can no longer regard electric and magnetic fluids attracting or repelling at a distance as realities, we may now also contemplate as a thing of the past that belief in atoms and in vacuum, against which Leibnitz so earnestly contended in his memorable correspondence with Dr. Samuel Clarke.

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* 'Dynamical Theory of the Electromagnetic Field,' October, 1864, § 20.

† 'Sir G. G. Stokes, Memoir and Scientific Correspondence,' vol. 2, pp. 1–45.

‡ 'Electrostatics and Magnetism,' p. 223, §§ 288–291.

§ In later years Lord Kelvin would have partially withdrawn this, finding it still necessary to form hypotheses about the field of force of an atom in the absence of knowledge of what the atom itself intrinsically consists of. *Cf.* 'Baltimore Lectures, 2 ed., 1904.

"We now look on space as full. We know that light is propagated like sound through pressure and motion. We know that there is no substance of caloric,—that inscrutably minute motions cause the expansion which the thermometer marks, and stimulate our sensation of heat,—that fire is not laid up in coal more than in this Leyden phial, or this weight, there is potential fire in each. If electric force depends on a residual *surface action,* a resultant of an inner tension experienced by the insulating medium, we can conceive that electricity itself is to be understood as not an accident, but an essence of matter. Whatever electricity is, it seems quite certain that electricity in motion IS *heat*;* and that a certain alignment of axes of revolution in this motion is *magnetism.* Faraday's magneto-optic experiment makes this not a hypothesis, but a demonstrated conclusion. Thus a rifle bullet keeps its point foremost; Foucault's gyroscope finds the earth's axis of palpable rotation; and the magnetic needle shows that more subtle rotatory movement in matter of the earth, which we call terrestrial magnetism—all by one and the same dynamical action.

"It is often asked, are we to fall back on facts and phenomena, and give up all idea of penetrating that mystery which hangs round the ultimate nature of matter? This is a question that must be answered by the metaphysician, and it does not belong to the domain of Natural Philosophy. But it does seem that the marvellous train of discovery, unparalleled in the history of experimental science, which the last years of the world has seen to emanate from experiments within these walls, must lead to a stage of knowledge, in which laws of inorganic nature will be understood in this sense that one will be known as essentially connected with all, and in which unity of plan through an inexhaustibly varied execution will be recognised as a universally manifested result of creative wisdom."

His studies in the doctrine of energy (*infra,* p. xxix) soon led Thomson into the intricate problem of the mechanical value of an electric current (*i.e.*, the *electrokinetic* energy), through which he successfully threaded his way. What he published is a brief statement in Nichol's 'Cyclopædia,' edition 1860, "Magnetism, Dynamical Relations of." He explains that the inertia concerned cannot be intrinsic inertia of the moving electricity, for Faraday found that a current doubled back on itself gave no sensible spark on breaking the circuit; yet, if such inertia were ever detected it could readily be included. It is the energy of the electric induction to which the inertia belongs, as Faraday himself recognised. He sees that when two currents, each sustained constant by an impressed electromotive force, are allowed to develope energy of mechanical work by their mutual attraction, their electrokinetic energy is also increased by an equal amount, both these amounts being provided from the energy of the battery. Thus, for example, the electrokinetic energy of a single circuit is the work that would have to be *done* to double it close upon itself.* He states that this energy is the volume integral throughout space of square of magnetic force divided by $8\pi$; an expression which Maxwell took later, with happy results, to represent the electrokinetic energy in distribution as well as in amount. In 'Elec. and Mag.,' 1872, p. 447, where this statement is reprinted in full in a footnote, he adds a most interesting memorandum of date October 13, 1851, which shows how he had teased it into form. He had thought "that the [mechanical] value of a current will be affected by steel [*i.e.*, permanent] magnets in its neighbourhood." But he was shaken in this by Faraday's having found that soft iron is better than steel. He "made out the true state of the case," which was that when a current is moved near a permanent magnet the gains and losses of energy compensate without demanding any alteration in its intrinsic energy due to change of its position. But when one maintained current is moved near another, the principle of conservation of total energy, electrical and thermal, requires that their electrokinetic energy shall *increase* by the mechanical work they perform in their change of position.

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* This view has been curiously revived in some recent theories of electric and thermal conduction.

This verification of the conservation of the total energy (October 1851) belongs to the time when Thomson had finally given his adhesion to Joule's doctrine that heat is energy instead of being a substance, and the development of thermodynamics was in full cry. Unknown to him, the same problem had been essayed in a tract which formed one of the highest efforts of genius applied to the development of the Theory of Energy, the famous 'Erhaltung der Kraft' of Helmholtz, published by him in 1847 to meet, as he said afterwards, with neglect from contemporary physicists (yet F. E. Neumann was then in his prime), redeemed, however, by the educated appreciation of the great mathematician Jacobi. Helmholtz had not then been under Faraday's influence, and could have no idea of energy stored in organised kinetic form in the magnetic field of the current; accordingly, when he puts down the equation of conservation for two mutually influencing currents, he gets it wrong.† For the case of a current and a permanent magnet it comes right, but it required Thomson's examination to prove that it is so.‡ Thus to Helmholtz belongs the merit of determining theoretically the constant of proportionality in Faraday's law of induction, by the aid of the conservation of energy: while Thomson's closer examination brought to light that the one equation of energy could determine only one variable, and thus prepared the way for Maxwell's application of the generalised dynamics of Lagrange, which came only in 1864.* Thomson had, however, as early as 1848, communicated to the British Association† an investigation based on Neumann's expression for the law of induction, and on the same lines as Helmholtz's exposition; he there speaks of $ki^2$ as "mechanical effect continually lost or spent in some physical agency (according to Joule, the generation of heat)."

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* This had been published in 'Proc. Glasgow Phil. Soc.,' January 1853. *Cf.* 'Math. and Phys. Papers,' vol. 1, p. 530.

† It was so quoted in Maxwell's 'Treatise,' vol. 2, § 544 (1872). It remains so in the reprint, Helmholtz's 'Abhandlungen,' vol. 1, p. 64 (1881); but in pp. 91–5 are some improvements of date 1854 as regards induction with magnetisable iron that were called forth by the criticism of Clausius.

‡ *Cf.*, however, Helmholtz's addition of 1854 in reply to Clausius, where he mentions his difficulty of access to electrical literature in 1847, and modestly places the chief merit of the essay in the point of view.

Thomson was not slow in developing the entrance to the mechanical relations of electric currents, and of electric inertia, thus obtained. The classical paper on transient currents was communicated to the Glasgow Phil. Soc. in January 1853,‡ and published in full in the 'Phil. Mag.' the following June. It contains the demonstration that oscillatory electric discharges must exist under suitable circumstances,—and gives that determination of their period which, in the hands of Hertz, led to the detection of electric waves in free space, bringing with them wireless telegraphy, and in the other direction nearly bridging the gap between electric experiments and optical phenomena. It appears from a footnote (p. 549) that he arrived at this theory early in 1852. He afterwards found that Helmholtz had definitely suspected the oscillatory character of the discharge, in the 'Erhaltung der Kraft' (1847), from its alternating effects in magnetisation as observed by Riess, and from the evolution of mixed gases in electrolysis which was discovered by Wollaston and at a later time puzzled Faraday.

We now pass to another phase of Thomson's mental activity. His first formal memoir—'in the grand style' as has been said—appeared§ in 1849, on "The Mathematical Theory of Magnetism." As he recounts in an abstract, in the magnetic theory of Poisson, employed by Green and by Murphy, the development is based on a hypothesis of two magnetic fluids, which the recent discoveries in electromagnetism had rendered incongruous. The aim of the memoir is to purify the expression of the theory by placing the results on a wider foundation. The general idea of polarity and of a polar element of volume is defined and made precise. "However different are the physical circumstances of magnetic and electric polarity, it appears that the positive laws of the phenomena are the same,|| and therefore the mathematical theories are identical. Either subject might be taken as an example of a very important branch of physical mathematics which might be called 'A Mathematical Theory of Polar Forces.'" The memoir proceeds with abundant explanation, perhaps needed at that time to supplant the cruder imagery, and to enforce the relation of Poisson's ideal density of magnetic matter to the actual distribution of polarity which it, only in certain respects, represents. He defines the *potential*, with due reference to Green's intro-

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* 'Dynamical Theory . . .,' §§ 17, 24 *seq.*

† 'Math. and Phys. Papers,' vol. 1, p. 81.

‡ 'Math. and Phys. Papers,' vol. 1, p. 534.

§ 'Transactions of the Royal Society' for June 1849, and June 1850.

|| Referring for this to 'Cambridge and Dublin Math. Journal,' vol. 1, 1845, as *supra*.

duction of the name in 1828, and shows that if that function is calculated as for the distribution of ideal magnetic matter, its gradient represents the magnetic *force,* defined as Maxwell used the term afterwards, to represent the force in a cavity of such elongated shape in the polarised medium that there is no sensible purely local part. In the Reprint in 1871 is here inserted, in illustration, an investigation of the centre and axes of a magnet, reproduced in Maxwell's Treatise (§ 392 *seq.*) the following year. Then the *mutual potential energy* of two magnets is formulated with a view to the determination of their attractions. A chapter follows on solenoidal and lamellar distributions of magnetism, and as a special case the Gaussian magnetic shell, which perhaps may be said to have expressed a theory of Ampère in the new geometrical terminology of solid angles. Then follow long disquisitions, partly interpolated in the Reprint from contemporary manuscripts, partly of date 1871, which confirm the impression that the writer's strength does not lie in synthetic exposition, but rather in flashes of insight and play of suggestion around his results such as have already been passed in review.

A footnote of 1872 explains that in 1850 he had no belief* in the reality of Ampère's theory of magnetism, not then knowing "that motion is the very essence of what has been hitherto called matter. At the 1847 Meeting of the British Association at Oxford, I learned from Joule the dynamical theory of heat, and was forced to abandon at once many, and gradually from year to year all other, statical preconceptions† regarding the ultimate causes of apparently statical phenomena." Then the introduction to his paper of 1856 on the dynamics of magneto-optics is quoted in full as already reproduced (*supra,* p. xvi).

At the end of the Reprint he inserts a paper, "On the Potential of a Closed Galvanic Circuit of any Form," of date 1850,‡ which, besides emphasising the energy-aspect of the potential, is concerned with definitions (suggested by some of De Morgan's on area) of solid angle for complex convoluted types of circuits, which is in fact a subject in *Analysis Situs.*

It will be convenient to follow out here the more recent additions on magnetic theory which make up the remainder of the Reprint. There is a chapter on the Mechanical Value (Energy) of Distributions of Matter and of Magnetism. Then follows a chapter on "Hydrokinetic Analogy" to Magnetic Flux, and a further paper at the end entitled "General Hydrokinetic Analogy for Induced Magnetism" (1872), which are in fact extensions of the beautiful representation by frictionally resisted flow through a solid porous mass, which Maxwell had employed, apparently unknown to him, with much elegance in his earliest memoir "On Faraday's Lines of Force." In the latter paper the new term 'permeability' (to flux) is defined as an equivalent for coefficient of induction.

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* "It is absolutely impossible to conceive of the currents which he (Ampère) describes, round the molecules of matter, as having a physical existence," 'Brit. Assoc.,' 1867 (Oxford); 'Elec. and Mag.,' p. 469.

† His first memoirs on vortex motion are of date 1867–9.

‡ 'Cambridge and Dublin Math. Journal.'

A chapter on 'Inverse Problems' in Magnetism is added, containing much detail on the analogy with flow, after the manner of his first paper of 1845. Here a characteristic passage is of personal interest. "With reference to these problems I find a leaf of manuscript written in French, indorsed:—'Fragment of draft of letter to M. Liouville, written on the Faulhorn, Sunday, September 12, 1847, and posted on the Monday or Tuesday week after, at Maidstone. The letter has not been published yet, although in September 1848, I understood from M. Liouville, in Paris, that he had it for publication. Probably it has fallen aside and is lost [? in consequence of the disturbed state of Paris at that time], which I should regret, as it contains my first ideas, and physical, especially hydrodynamical, demonstrations of the theorems I am now about to write out for my paper on "Magnetism" for the Royal Society, from rough drafts written in August 1848. W. T., October 29, 1849.'* The 'now' has been deferred until the present time, November 20, 1871. I am obliged to write from memory, as I have not been able to recover any of those rough drafts . . . . ." The chapter consists largely of theorems of existence and determinacy of magnetic distributions, corresponding to assigned conditions over the boundary of the region, which are established and enforced from the analogy of liquid flux.

To the British Association at Oxford, in 1867, he explained concisely† and on modern lines how all the phenomena of terrestrial magnetism could be represented as the effect of a calculable ideal sheet of electric current spread over the surface, whether spherical or otherwise.

The continuation of the memoir on Magnetism of 1849–50, which treated of the "Theory of Magnetic Induction in Crystalline and Non-crystalline Substances," overflowed into another Journal‡ in 1851. He begins by recounting how Poisson in his third memoir on Magnetism (1823) had already contemplated the effect of crystalline arrangement of the 'magnetic elements' within which the neutral 'magnetic fluid' was considered to be separable by the field so as to produce polarity: non-sphericity of form (or any æolotropy) would also be potent, for the axes of the elements would all be similarly orientated. The subject was dropped by Poisson, after arriving at the linear vector form of relation, of unrestricted type, however, connecting the induced magnetisation and the magnetising force: he remarks that it would be curious to test whether crystallised substances actually exhibited such directional effects. Thomson points out that "a recent discovery of Plücker's had established the very circumstance," which now makes obvious the importance of working out a mathematical theory. As in the earlier memoir, a main object is to replace the artificial conception of a magnetic fluid by distribution of magnetic polarity, which is all that is objectively ascertainable, combined with the hypothesis of simple superposition of effects—restricted, however, to feebly magnetic material, to ensure that the equations of magnetisation are thus linear. Then he considers the magnetisation of a small crystalline sphere situated in a uniform field. He asserts the existence of three principal magnetic axes in the crystal, which he is tempted in a footnote to identify with the known principal axes of optical elasticity. He expresses the torque acting on such a sphere, by aid of the hypothesis of superposition of magnetisations due to the components of the inducing field along these principal magnetic axes. Then he gets to the expression of a work-function depending on position and orientation, in the various spacial gradients of which the translational forcive is involved as well as this rotational forcive.

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* 'Elec. and Mag.,' p. 458.

† 'Elec. and Mag.,' pp. 468–70.

‡ 'Phil. Mag.,' March 1851; 'Elec. and Mag.,' pp. 471–87.

As he now remarks, he had recently obtained* this work-function for the simpler case of isotropic material in the form $\frac{1}{2}\kappa H^2.\delta$vol., and established by means of it the theoretical validity of Faraday's principle derived from observation, the tendency of magnetisable matter to travel towards regions of more intense force. He now extends it to the general case. He inquires of Faraday whether he had noticed that a piece of bismuth was repelled differently according to its orientation, a question already suggested by Poisson at the very beginnings of the subject. Then he cites Faraday's suggestion of two years before (December 1848), assigning inductive quality, varying with direction, as the cause of the definite orientation of a small crystalline mass near a powerful magnetic pole, which shows that Faraday had propounded the same question for himself. The question was immediately answered, in the manner anticipated, in experiments of Tyndall. Then he throws out a suggestion of the curious results obtainable with a crystal, immersed in fluid of inductive power intermediate between its own greatest and least crystalline inductive powers.

An appendix to this paper makes the quotations from Poisson's memoir referred to above. Then it proceeds to a remark which illuminates the whole subject. After reporting Poisson's general linear vector relation between magnetisation and force, involving nine coefficients reducible to one for isotropic matter, he proceeds as follows:—"and there is nothing to indicate the possibility of establishing any relations among the nine coefficients which must hold for matter in general. I have found that the following relations, reducing the number of independent coefficients from nine to six, must be fulfilled, whatever be the nature of the substance [namely, equality of the conjugate diagonal coefficients]; the demonstration being founded on no uncertain or special hypothesis, but on the principle that a sphere of matter of any kind, placed in a uniform field of force, and made to turn round an axis fixed perpendicular to the lines of force, cannot be an inexhaustible source of mechanical effect. All the conclusions with reference to magnecrystallic action enunciated in the preceding abstract are founded on these relations."

There the subject breaks off in 1851: something else had obtained possession of the author's mind. Demonstration and elucidation are provided in the Reprint in 1872. The result was, perhaps, one of the most exquisite and brilliant mathematical applications* of the principle of the Conservation of Energy that had yet or ever been made: not only did it show that every crystal must have three principal axes of magnetic induction, without any rotational quality, but the argument is also directly applicable to electrostatic induction, and is thus the essential feature in the immediate deduction by Maxwell thirteen years later of Fresnel's laws of optical double refraction from electric principles.

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* 'Phil. Mag.,' October 1850; but see next page, referring to 'Cambridge and Dublin Math. Journal,' 1847.

It may be recalled again in connexion with the above that Faraday's doctrine of flux in tubes of force and "conducting power for lines of force" dates from October 1850, and thus comes between Thomson's two memoirs described above.

It should also be recorded that the explanation of Faraday's principle, that a small soft iron sphere is urged towards regions of stronger force, belongs to a date as early as 1847.† But then the force urging the sphere is proved to be the gradient of the as yet uninterpreted function $\frac{1}{2}\kappa H^2$ vol., obtained by direct calculation from the magnetic principles of Poisson; the remark that it was not until the autumn of 1847 that he had learnt the doctrine of Conservation of Energy from Joule has been quoted already. Helmholtz's 'Erhaltung der Kraft' appeared in July 1847.

Yet he triumphantly utilises this force-function to vindicate Faraday's profound view that a thin bar of diamagnetic material should point equatorially when placed in the line between magnetic poles. In a uniform field it *must* point axially, though with a force extremely feeble; but, quoting the words of Faraday,‡ "the cause of the pointing of the bar, of any oblong arrangement of the heavy glass, is now evident. It is merely a result of the tendency of the particles to move outwards, or into the positions of weakest magnetic action. The joint exertion of the action of all the particles brings it into the position which by experiment is found to belong to it." This doctrine proved difficult, and, in fact, became controversial, to some physicists unduly dominated by the simpler phenomena of forces of orientation, even Plücker's previous conclusions not being unexceptionable; and a good deal of attention was paid by Thomson at this time to its further elucidation and to very fascinating experimental illustrations. It is in a sense, possibly already in Faraday's own view, the generalisation of the hydrostatic principle of Archimedes, which would assert that in a field of power a more susceptible body will displace one less susceptible. The subject need not here be followed further.

Thomson and Maxwell both revert again and again to this crowning instance of Faraday's mathematical sagacity. Thus, to Thomson, in 1870,§

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* 'Elec. and Mag.,' p. 485.

† 'Cambridge and Dublin Math. Journal,' vol. 2, dated from Peterhouse, May 13.

‡ 'Exp. Res.,' No. 2269.

§ 'Elec. and Mag.,' p. 580.

"One of the most brilliant steps made in philosophical exposition of which any instance existed in the history of science, was that in which Faraday stated, in three or four words, intensely full of meaning, the law of the magnetic attraction and repulsion experienced by inductively magnetised bodies." And again, "Mathematicians were content to investigate . . . .; but Faraday, without mathematics, divined the result of the mathematical investigation; and, what has proved of infinite value to the mathematicians themselves, he has given them an articulate language in which to express their results. . . . It must be said for the mathematicians that they greedily accepted it, and have ever since been most zealous in using it to the best advantage."

Incidentally it is of interest to note that Thomson's 'theoretical' physical solution of the problem of 'Mahomet's coffin,' to suspend a body in stable equilibrium in mid-space without supports or contacts of any kind, appears in this paper, the example being a diamagnetic sphere situated on the axial line of a straight vertical electromagnet.

If one had to specify a single department of activity to justify Lord Kelvin's fame, it would probably be his work in connexion with the establishment of the science of Energy, in the widest sense in which it is the most far-reaching construction of the last century in physical science. This doctrine has not only furnished a standard of industrial values which has enabled mechanical power in all its ramifications, however recondite its sources may be, to be measured with scientific precision as a commercial asset; it has also, in its other aspect of the continual dissipation of available energy, created the doctrine of inorganic evolution and changed our conceptions of the material universe. A sketch of the early history of this doctrine will illustrate the innate power and independence of Lord Kelvin's thought, as well as in some degree his relations to his great predecessors and contemporaries.

The initial difficulty of the subject lay in the feature, entirely novel to physical science, that in the inorganic world what we call dissipation or scattering of energy is loss only in a subjective sense; it concerns only the energy "available *to man*, for the production of mechanical effect," to use Thomson's own phrase of 1852.* We can produce organised mechanical effect from diffuse energy such as heat, which consists in the unregulated motion of a crowd of jostling molecules, only by judicious guiding of its innate effort towards an equilibrium, just as we can get power from a turbulent waterfall by guiding the stream against a mill-wheel or turbine. But when the average of the molecular motions has come to a steady equilibrium throughout all parts of the material system, of which uniformity of temperature is the criterion, all chance of arranging or guiding part of its molecular energy into co-ordinated power available for our operations on finite bodies has passed away. This is, roughly, the *rationale* of the principle of Carnot. Yet the energy has not disappeared; it is still there, but it is uniformly diffused and so not recoverable into the organised form of mechanical power. This absolute conservation of the total energy is the principle of Joule, which is the main experimental support of the presumption that all energy is ultimately of the dynamical type. In a complete view of physical transformations the two principles, of Carnot and of Joule, have both to find their places. Here a fundamental perplexity confronted and detained Lord Kelvin for some three years, 1847–50.* When heat is allowed to flow away to a lower temperature without passing through an engine, its capacity for doing work has been dissipated. The opportunity for obtaining mechanical power from it has vanished beyond recall. Can then heat be correctly measurable as mechanical energy if some of the mechanical energy is lost irrecoverably every time that the heat diffuses to a lower temperature? Thomson, ever attracted by the engineering side of things, was dominated by Carnot's principle, as we have seen, even when as a youth, in 1845, he went to Paris to Regnault's laboratory. Thus he at once set himself to explore its practical content by the aid of the mass of exact data on gases acquired by Regnault, as soon as these results appeared, in 1847, as the first instalment of the famous series of experimental researches, which had been subsidised by the French Government with a view to obtaining all the data that could be pertinent towards the improvement of knowledge of the principles of steam and gas engines. In Thomson's first paper† towards this end, entitled "On an Absolute Thermometric Scale founded on Carnot's Theory of the Motive Power of Heat, and calculated from Regnault's Observations," he clears the ground for exact physical reasoning by elevating the idea of temperature from a mere featureless record of comparison of thermometers into a general principle of physical nature, making it a measure of the dynamical potentiality of heat, which is, on Carnot's principles, an intrinsic measure, *i.e.*, quite independent of the substances in which the heat happens to be contained. But he cannot get rid of the impression that heat is something different from energy, which may produce energy in falling to a lower level of temperature, or on the other hand may diffuse passively, so that this opportunity of creating energy is irrecoverably wasted. Such a view would tend towards the caloric theory which held that heat is somehow substantial; in terms of it Carnot, in fact, formulated his argument. It has been remarked on this by Helmholtz that if Carnot had then possessed completer knowledge he would possibly never have hit upon his principle; on the other hand, his rough manuscripts, published many years after, have revealed that during the remaining six years of his short life he was inclining strongly towards the correct view on the nature of heat. In a footnote, Thomson gives expression to his own doubt. The experiments of "Mr. Joule, of Manchester," seem "to indicate an actual conversion of mechanical effect into caloric. No experiment, however, is adduced in which the converse operation is exhibited; but it must be confessed that as yet much is involved in mystery with reference to these fundamental questions of Natural Philosophy." And in a fuller account, soon after, of Carnot's Theory,* as further developed numerically by aid of the data given by Regnault's experiments on steam, he adheres substantially to this position, "although this, and with it every other branch of the Theory of Heat, may ultimately require to be reconstructed upon another foundation when our experimental data are more complete." He returns, in a note, stimulated by a remark of Joule, to the problem of what becomes of the mechanical effect that appears to be lost when heat diffuses; but he cannot admit the suggestion of Joule to cut the knot by abandoning Carnot's principle, and he appeals to further experiment "either for a verification of Carnot's axiom, and an explanation of the difficulty we have been considering: or for an entirely new basis for the Theory of Heat." Still harassed by these doubts, he returns yet again to test the experimental verification of Carnot's principle (which he finds adequate) in an Appendix;† for, as he says, "Nothing in the whole range of Natural Philosophy is more remarkable than the establishment of general laws by such a process of reasoning" as is that principle in its wider ramifications.

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* 'Math. and Phys. Papers,' vol. 1, p. 505.

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* *Cf.* Osborne Reynolds' very illuminating "Life of Joule," forming vol. 6, 1892, of 'Mem. Manchester Lit. and Phil. Soc.' After the present notice had been prepared, the writer found that the early history of thermodynamics had been gone over by Professor E. Mach, of Vienna, in 'Die Principien der Wärmelehre historisch-kritisch entwickelt,' 1896, with results, in its restricted range of applications purely thermal, which seem to agree substantially with the views here taken. The development of the ideas and formulæ of the general science of energetics (*infra*, p. xlvii) by Thomson is thus not considered; while in most other special treatises it is almost entirely obscured by departure from the historical order of exposition.

† 'Proc. Cambridge Phil. Soc.,' June 1848.

We have here found Thomson actually hesitating as to whether heat is to be classified as energy, on the ground that the fall of heat to a lower temperature can occur without developing any mechanical work. Yet it is true, as Lord Rayleigh has expressed it,‡ that most great authorities, especially in England, including Newton, Cavendish, Rumford, Young, Davy, etc., have always been in favour of the doctrine that heat is a mode of motion. The fact is, as we have seen, that Thomson knew too much to allow him to rest in such a partial view of things; he saw, also, a totally different side of the subject, which not even his close connexion with Joule, and appreciation of his work, could allow him merely to ignore.

Just a year before Thomson's first paper on Carnot's principle, Helmholtz, then a young army surgeon, had stepped (1847) into the first rank of physicists (though recognition came later, the memoir, *e.g.*, becoming known to Thomson only in 1852) by the publication of the 'Erhaltung der Kraft,' which asserted the universality of the conservation of total energy, and developed with convincing terseness and lucidity the ramifications of that principle throughout nature. To establish the transformation of heat into work he is already able to appeal to the classical experiments of Joule, published three years previously (1844)—not yet mentioned by Thomson, whether it was from want of knowledge or from some fancied mode of evading their force in the light of his insistence on Carnot's principle. These experiments proved definitely that expansion of a gas working against the pressure of the atmosphere absorbs an equivalent of heat, whereas expansion into a vacuum absorbs none. It was, in fact, in this paper that Joule rather summarily condemned Carnot's principle as above mentioned, on account of its supposed discrepancy with his own established results. And Helmholtz had naturally to consider this point. He seems to have had access then only to Clapeyron's account of Carnot, of date 1843, from which, however, he expounds the argument succinctly and correctly. He admits the probability of the truth of Clapeyron's deductions for gases, but falls back on the suggestion that they may also be obtainable otherwise on more certain principles; while he characterises as very unlikely the (correct) inference that compression of water between its point of maximum density and the freezing-point would absorb heat. Thus Helmholtz,* contrary to Thomson, saves the conservation of total energy by abandoning and ignoring the ideas belonging to the principle of Carnot.

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* 'Trans. R. S. Edinburgh,' January 2, 1849.

† April 30, 1849.

‡ "The Scientific Work of Tyndall," 'Roy. Inst. Proc.,' March 16, 1894.

The brilliant and suggestive writings of J. R. Mayer on the conservation of total energy were at that time unknown to Helmholtz: they seem to have been first brought to general notice† by Joule himself in the classical memoir on the Mechanical Equivalent of Heat presented by Faraday to the Royal Society in 1849. The sketch above given will have shown how little such theoretical considerations as those of Mayer, however illuminating and acute within their own range, were calculated to remove the profounder perplexities of Thomson, so long as there remained the apparently essential contradiction on which these doubts had their foundation. His insistence in class lectures on the absolute necessity for Joule's experimental work is still recalled by his students.

The credit of being the first to resolve these difficulties belongs to Clausius. In his memoir "On the Motive Power of Heat and the Laws of Heat which may be deduced therefrom," communicated to the Berlin Academy in February 1850, he quotes the title of Carnot's tract (Paris, 1824) in a footnote at the beginning‡ of the paper, which proceeds as follows:—"I have not been able to procure a copy of this work: I know it solely through the writings of Clapeyron and Thomson, from which latter are taken the passages hereafter cited." Then, in the introductory section, after referring to the difficulties above discussed, and the work of Holtzmann, Mayer, and Joule, he continues:—

"The difference between the two ways of regarding the subject has been seized with much greater clearness by W. Thomson, who has applied the recent investigations of Regnault, on the tension and latent heat of steam, to

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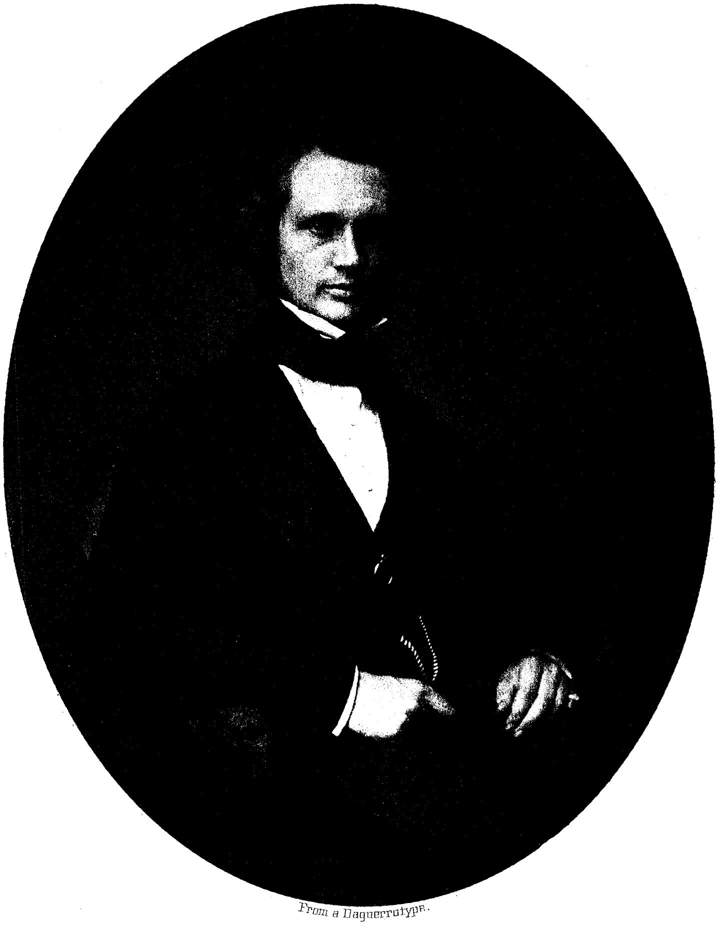
* 'Wissenschaftliche Abhandlungen,' vol. 1, p. 38.

† Osborne Reynolds, *loc. cit.*, p. 133.

‡ The quotations are from Hirst's translation, in which this memoir occupies pp. 14–68.



From a Daguerrotype.

Yours always truly
William Thomson

1854

the completing of the memoir of Carnot.* Thomson mentions distinctly the obstacles which lie in the way of an unconditional acceptance of Carnot's theory, referring particularly to the investigations of Joule, and dwelling on one principal objection to which the theory is liable. If it be even granted that the production of work, where the body in action remains in the same state after the production as before, is in all cases accompanied by a transmission of heat from a warm body to a cold one, it does not follow that by every such transmission work is produced, for the heat may be carried over by simple conduction; and in all such cases, if the transmission alone were the true equivalent of the work performed, an absolute loss of mechanical force must take place in nature, which is hardly conceivable. Notwithstanding this, however, he arrives at the conclusion that in the present state of science the principle assumed by Carnot is the most probable foundation for an investigation on the moving force of heat. He says: 'If we forsake this principle, we stumble immediately on innumerable other difficulties, which, without further experimental investigations, and an entirely new erection of the theory of heat, are altogether insurmountable.'

"I believe, nevertheless, that we ought not to suffer ourselves to be daunted by these difficulties; but that, on the contrary, we must look steadfastly into this theory which calls heat a motion, as in this way alone can we arrive at the means of establishing it or refuting it. Besides this, I do not imagine that the difficulties are so great as Thomson considers them to be; for although a certain alteration in our way of regarding the subject is necessary, still I find that this is in no case contradicted by *proved facts.* It is not even requisite to cast the theory of Carnot overboard; a thing difficult to be resolved upon, inasmuch as experience to a certain extent has shown a surprising coincidence therewith. On a nearer view of the case, we find that the new theory is opposed, not to the real fundamental principle of Carnot but to the addition 'no heat is lost'; for it is quite possible that in the production of work both may take place at the same time; a certain portion of heat may be consumed, and a further portion transmitted from a warm body to a cold one; and both portions may stand in a certain definite relation to the quantity of work produced. This will be made plainer as we proceed; and it will be moreover shown that the inferences to be drawn from both assumptions may not only exist together, but that they mutually support each other."

This memoir, as Willard Gibbs justly claims in his obituary notice (1889) of Clausius, laid securely the foundations of modern thermodynamics. But it seems equally true that this high merit lies mainly in the single remark at the end of the passage just quoted, which resolved the difficulties that had stopped Thomson; after that the development, though luminously accomplished, would have been plain sailing to any first-class intellect. Thomson's great memoir "On the Dynamical Theory of Heat,"† in which he at once connects Clausius' name with that of Carnot, appeared the following year. After giving a demonstration of the principle of "Carnot and Clausius" (§ 13), he proceeds (§ 14) to say that, about a year before, he had adopted this principle in connexion with Joule's principle, notwithstanding that he could not then resolve the apparent discrepancy, as the basis of a practical investigation of the motive power of heat in air and steam engines. "It was not until the commencement of the present year that I found the demonstration given above . . . . It is with no wish to claim priority that I make these statements, as the merit of first establishing the proposition upon correct principles is entirely due to Clausius, who published his demonstration of it in the month of May last year, in the second part of his paper on the motive power of heat. I may be allowed to add that I have given the demonstration exactly as it occurred to me before I knew that Clausius had either enunciated or demonstrated the proposition . . . . The reasoning in each demonstration is strictly analogous to that which Carnot originally gave."

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* 'Trans. R. S. Edinburgh,' vol. 16.

† 'Trans. R. S. Edinburgh,' March 1851.

Once Thomson gets thus under weigh, as we have seen, by his own unaided efforts though anticipated by Clausius, he developes rapidly the thermal aspects of the subject, concurrently with Clausius and Rankine, but with wider generality, in particular avoiding their hypotheses connected with perfect gases. So little was he prepared to trust to a permanent gas thermometer as giving practically the intrinsic dynamical scale of temperature, that the following year he had already begun with Joule their series of laborious joint experiments to determine exactly how much the gas thermometers differ from the absolute scale. Their procedure was to deduce the result sought from observation of the slight cooling or heating produced by driving the gas under high pressure through a porous partition; with a perfect gas the process would be isothermal. When we consider that the results were to lead straight into the very core of molecular dynamics, the investigation may well rank to this day as one of the most striking advances in the record of physical science. It is noteworthy that Thomson in his own work kept on with the symbol for the unknown Carnot's function, until the dynamical scale had thus been experimentally investigated; though a gas thermometer was doubtless adequate to give to Clausius and Rankine indications of absolute temperature, so far as required for their preliminary approximate investigations over limited range. We have only to think of the modern physical undertakings steadily pushed downward toward the absolute zero of temperature, to realise that, except on the basis of Thomson's dynamical scale of 1847 and his method conjointly with Joule of exactly realising it in 1852, there could be no such thing as temperature in a scientific sense, and low temperature research would be devoid of most of its significance. These essential foundations for the scientific treatment of Energy were laid firmly in 1852, in a way that has held good without substantial modification ever since.

Perhaps this point, the rigorous scientific generality of the foundations on which he built from the beginning, could not be enforced more strongly than by recalling that it is just this Thomson-Joule intrinsic cooling effect of expansion without external work, very slight under ordinary conditions, due merely to mutual separation of the molecules of the gas, that is the essential feature in the modern continuous processes for liquefaction of even the most refractory gases, by the expenditure of mechanical power to abstract the heat, which have now become familiar. On the other hand, the great economy of the reversed Carnot gas-cycle for ordinary refrigeration was pointed out in 1852, and applied by his brother to the ventilation of Belfast College.

In their parallel developments of the subject, while Clausius kept mainly to the theory of heat engines, applications over the whole domain of physical science crowded on Thomson. Already in December 1851, he communicates to the Royal Society of Edinburgh his Theory of Thermo-electric Phenomena, including the classical prediction of the convection of heat by the electric current, the so-called Thomson effect, which in the theory of electrons has a literal title to its original name. The formulæ of the printed abstract* of this paper show that he must have been already in full command (December 1851) of Carnot's principle in its most generalised form,—viz., as he expressed it in May 1854, but there introducing absolute temperature T, then recently determined by himself and Joule,—that in a complete reversible cycle of change $\Sigma(\mathrm{H}/\mathrm{T})$ vanishes, or in differential notation $\int(d\mathrm{H}/\mathrm{T}) = 0$, a form which was independently given by Clausius in December 1854, and from which the transition to Clausius' entropy-function (1856) is but a step. These advances appeared in full in the memoir, 'Trans. R. S. Edin.,' 1854,† where, in the way customary with him, he passes on to a long digression on the thermo-electrics of crystalline matter, including, after Stokes, the full theory of rotational vector effects. This latter subject was brought again into prominence many years after, when times were riper for it, with reference back to the present exposition, on the announcement by E. H. Hall of the discovery of an influence of this kind in electric conduction in a powerful magnetic field. Here also shines forth in a notable example what was always a main feature of Thomson's theoretical activity, the utilisation to the utmost of models and images of physical phenomena. He absolutely refused to deny to matter, however continuous and uniform as to sense it might appear to be, the possession of any property which he could imitate in a lattice structure or other architectural model, however complex; clearly, in his view, one has no right to assign limits *a priori* to the possible physical complexity of molecular aggregation.

One type of such limits, indeed, the only ones *a priori*, he vindicated in one of his most refined theoretical advances, those, namely, which are imposed on reversible phenomena by the principle of the conservation of energy. The demonstration on these lines that there can be no rotational quality in either magnetic or dielectric excitation in continuous media afterwards became, in Maxwell's hands, one of the main confirmations in the general electric interpretation of optics, by leading at once to the validity of Fresnel's theory of double refraction. *Cf. supra*, p. xxviii.

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* 'Math. and Phys. Papers,' vol. 1, pp. 316–323.

† *Loc. cit.*, pp. 232–261.

But we must return from this digression. The cosmical aspect of Carnot's principle, in its reconcilement with that of Joule, had immediately arrested Thomson's attention, and the fundamental law of Dissipation of Energy in natural phenomena stood revealed in a brief note in April 1852, embodying the following momentous and carefully formulated conclusions* :—

"1. There is at present in the material world a universal tendency to the dissipation of mechanical energy.

"2. Any *restoration* of mechanical energy, without more than an equivalent dissipation, is impossible in inanimate material processes, and is probably never effected by means of organised matter, either endowed with vegetable life or subject to the will of an animated creature.

"3. Within a finite period of time past, the Earth must have been, and within a finite period of time to come the Earth must again be, unfit for the habitation of man as at present constituted, unless operations have been, or are to be, performed, which are impossible under the laws to which the known operations going on at present in the material world are subject."

It is of interest to contrast this principle of degradation, or diffusion, of energy towards a uniform equilibrium, with the other great principle, dominating the phenomena of the organic world, which took shape at about the same time. Just fifty years ago biological thought was startled with the idea of the gradual evolution of organic forms, by the persistence, through hereditary transmission, of such accidental modifications as are adapted to the surrounding conditions of life, to the existing environment. In inorganic phenomena the energy becomes distributed among merely passive molecules; in the organic world the unit of investigation is an organism which has apparently the active property of fixing and transmitting in its descendants any structural peculiarity that it may come by. But even here there is something in common; the automatic evolution towards improved adaptation, in this case with no limit or equilibrium yet in sight, is attained at the cost of compensating dissipation, namely, the destruction of the individuals that happen to be ill adapted even though in other respects superior.

We observe in passing that in Thomson's formulation, Clause 2 already implies Clausius' conception (1854) of compensating transformations. What is perhaps now more interesting is that it expresses a decided opinion (which he still retained in 1892) on a question which Helmholtz† to the end preferred to leave open, namely, whether the refinements of minute structure and adaptation in vital organisms may permit departure from the law of dissipation, which is known to be inflexible in the inorganic world, by utilising to some extent diffuse thermal energy for the production of vital mechanical power. The development of Clause 3 led to the famous series of investigations and discussions regarding the beginnings and the ultimate fate of our universe, and the duration of geological time, which have formed a region of intimate contact, but not always of agreement, between dynamical and evolutionary science.

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* *Loc. cit.*, p. 514.

† See an interesting passage in his Lectures on Heat, posthumously published.

Earlier in the same note, and also more fully in 'Phil. Mag.,' February 1853, Thomson illustrated his early complete grasp of all matters relating to the availability of thermal energy and to compensating transformations, in calculating the dissipation which arises from throttling steam, and the work which can theoretically be gained from the thermal energy in an unequally heated space.

This history is, however, not yet complete. Examination of the 'Notes inédites' of Sadi Carnot, appended to the reprint of the 'Réflexions,' published with charming biographical detail by his brother in 1878, and welcomed enthusiastically by Lord Kelvin, leaves an impression that Carnot was already struggling with difficulties of the kind to which the insight of Thomson exposed him some twenty years later. He had analysed (p. 91), with sure instinct, the Gay-Lussac experiment concerning heat of expansion of gas by efflux, and afterwards developed it (p. 96) into a suggestion of the identical porous plug experiment of Joule and Thomson. He points out (p. 92) that the view that heat is "le résultat d'un mouvement vibratoire des molecules" conforms to our knowledge in a long list of the principal transformations of energy; "mais il serait difficile de dire pourquoi, dans le développement de la puissance motrice par la chaleur, un corps froid est nécessaire, pourquoi, en consommant la chaleur d'un corps échauffé, on ne peut pas produire du mouvement." He seems to be trying (p. 94) to think out a definite distinction between this movement of the particles of bodies and the "puissance motrice" into which it cannot be changed back. "Si les molecules des corps ne sont jamais en contact intime les unes avec les autres, quelles que soient les forces qui les séparent ou les attirent, il ne peut jamais y avoir ni production, ni perte, de puissance motrice dans la nature. Alors le rétablissement d'équilibre immédiat du calorique et son rétablissement avec production de puissance motrice seraient essentiellement différents l'un et l'autre." "La chaleur n'est autre chose que la puissance motrice, ou plutôt que le mouvement qui a changé de forme. C'est un mouvement dans les particules des corps. Partout où il y a destruction de puissance motrice, il y a, en même temps, production de chaleur," and reciprocally. Like Thomson at the later date, he intended to seek the guidance of further experiment, outlines of which he sketched. These extracts suggest the very problems which are still fundamental in the molecular theory of energetics, about which much is yet to be learned, though Thomson's theory of dissipation of energy and its molecular interpretation by Maxwell and Thomson and Boltzmann has illuminated the whole field. Yet Carnot already saw (p. 93) that his negation of perpetual motion demands that when heat does work in falling to a lower temperature, if some heat is really absorbed in the process the amount so absorbed must be independent of the mechanism of the process, must, in fact, be an equivalent of the work; for if the other alternative were possible, " on pourrait créer de la puissance motrice sans consommation de combustible et par simple destruction de la chaleur des corps." Clausius and Thomson had nothing in 1850 to add to this reasoning of date earlier than 1832.

No apology is required for thus dwelling at length on this episode in the evolution of the principles of physical science, the development of the principle of energy into its wider aspect, in which it assumes its universal co-ordinating *rôle* as the principle of available energy,—involving its complete available conservation only in the limited class of phenomena that satisfy the Carnot test of being reversible, and in other cases emphasising the partial dissipation into diffused unavailable molecular energy which is characteristic of the operations of physical nature. No passage in the history of modern physics can, perhaps, compare with it in interest. In the other outstanding advance of the last century, the unravelment of the function of the æther as the sole means of intercommunication between the molecules of matter so as to constitute a *cosmos,* as the seat of the activities of radiation and of electric and chemical change, the problem to be solved was of a different type. The questions have there been more precise; they have suggested, and their investigation has been directed by, definite adaptable trains of experiment. But the pioneers in the theory of available energy had to probe among the *arcana* of common experience, in a manner which takes us back to the beginnings of dynamical science and recalls the efforts of Archimedes and Galileo and Pascal in detecting controlling principles in the maze of everyday phenomena.

The original stimulus to all this wide grasp of the relations of inanimate nature had its origin in the progress of mechanical invention, in the successful construction and operation of thermal engines. Irrespective of the problem of their industrial improvement, the detection of the essential features of this mechanical value of heat would appeal strongly to an analytical mind like that of Carnot. But his compact informing principle, as its content was ultimately developed in Thomson's hands, far transcended the special thermal problem from which it started; it now dominates the whole range of physical science. It is only on its validity that our confidence is based, that we can treat the interactions of the finite bodies of our experience by strict mathematical and dynamical reasoning, entirely leaving aside, as self-balanced and inoperative, those erratic though statistically regular motions of the molecules, forming a very considerable part of the total energy, which constitute heat in equilibrium.

This fundamental basis of our knowledge of inanimate nature, thus acquired from clues suggested by human industrial improvements, still retains an aspect essentially anthropomorphic; it is conditioned by the limitations of our outlook as determined by the coarseness of our senses, as Maxwell seems to have been the first definitely to perceive. For the case of an ultra-material sentient creature of bodily size so small as to be comparable with a single chemical atom, his own sensible physical universe would be controlled by some fundamental law possibly of quite different type, while the phenomena which are prominent to us would take on for him a cosmical character as regards both time and space. We can ourselves catch partial glimpses of such a transformed physical universe, not subject to ordinary laws of matter in bulk, in the phenomena of high vacua, where the gaseous molecules come nearly individually before our attention and can almost be counted, and in the recent cognate phenomena of radio-activity either spontaneous or electrically excited. The boundary of demarcation of this new world from the universe which is dominated by the principle of available energy is naturally ill-defined: its exploration sheds light on both, and is perhaps the most interesting of the present activities of theoretical and practical physics.

Here also Lord Kelvin has played a part. Already, in 1852, he had prefixed to one of his papers the title "On the Sources available *to Man* for the Production of Mechanical Effect," as if in anticipation of this anthropomorphic side of the subject, first broached apparently by Maxwell in 1871 at the end of his "Theory of Heat," where he points out that it is only man's inability to obstruct passively the individual molecules at will that prevents the whole of their energy from being available, and shows how sentient agents capable of doing this could reverse the otherwise irrevocable course of diffusion of the energy in a gaseous medium.

Perhaps Thomson's own most systematic pronouncement on the inner significance of these relations is a short paper in 'Proc. R. S. Edin.'* of date February 1874. He points out that the changes contemplated in abstract dynamics are strictly reversible; while in actual physical phenomena the absence of reversibility is conspicuous, a fact which was already embedded in the principle of dissipation of energy in 1852. Now "the essence of Joule's discovery that heat is diffused energy is the subjection of physical phenomena to dynamical law." Yet if we could reverse all inanimate motion, inorganic nature would unwind again its previous evolution; "and if the materialistic hypothesis of life were true, living creatures could grow backwards with conscious knowledge of the future but no memory of the past, and would again become unborn. But the real phenomena of life infinitely transcend human science, and speculation regarding consequences of their ultimate reversal is utterly unprofitable. Far otherwise, however, is it in respect to the reversal of the motions of matter uninfluenced by life, a very elementary consideration of which leads to the full explanation of this theory of dissipation of energy."†

He goes on to explain in graphic terms how an army of Maxwell's

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* Also 'Nature,' vol. 9, 1874, pp. 421–424; 'Phil. Mag.,' March 1892, pp. 291–299.

† *Cf.* Helmholtz's review already quoted, 'Nature,' vol. 32, 1885, 'Wiss. Abhandlungen,' vol. 3, p. 594.

'demons'[*] stationed along an interface across a column of gas could produce various kinds of physical effect at will, either (i) establish a difference of pressure at the two sides without alteration of temperature, or (ii) *vice versâ,* while the dynamical possessions of the system as a whole, the *total* energy and momentum, remain absolutely unchanged.

Then he considers a volume of gas, having initially different temperatures in different parts, and allowed to pass towards the ultimate uniformity which constitutes equilibrium of temperature, by inter-diffusion of the molecules, each of them carrying its energy and momentum. After a certain time imagine the velocities of all the molecules to be exactly reversed, and the crowd will retrace their previous course, the diffusion being thus undone, ultimately attaining again the initial state, after which however diffusion towards uniformity will again supervene. How are we to reconcile this uncompensated restoration of energy with the universality of the principle of degradation? "The number of molecules being finite, it is clear that small finite deviations from absolute precision in the reversal we have supposed would not obviate the resulting dis-equalisation in the distribution of energy. But the greater the number of molecules the shorter will be the time during which the disequalising will continue; and it is only when we regard the number of molecules as practically infinite that we can regard spontaneous disequalisation as practically impossible." Then he proposes a problem of probability concerning a region of gas containing a finite number of molecules, viz., to determine the chance that say nine-tenths of them shall be in the upper half of their enclosure and only one-tenth in the lower half at any instant. This is solved (readily) by aid of the Theory of Probability in an appendix. It is shown that however great the number of molecules be, provided it is finite, a time must ultimately arrive when an excess in any assigned ratio, either of molecules or of energy, is in the upper half of the region. If a region contain $2.10^{12}$ molecules of oxygen and $8.10^{12}$ of nitrogen, the chance of their being entirely separated so that the oxygen is in a specified fifth part of the vessel is merely about one against ten raised to the power $2,173,220.10^6$. On the other hand, if there are $n$ molecules of the one gas and $n'$ of the other, the distribution in a partial region which is a fraction $e$ of the total space is exactly in the average proportion, that of $n$ to $n'$, without any molecule of either kind in excess in that part, only for a fraction $\{2\pi e(1-e)n^{\frac{1}{2}}n'^{\frac{1}{2}}\}^{-1}$ of the time: for the case above specified this is the fraction $(4021.10^9)^{-1}$ of the total time, that is, only one second in five million years.

The latter illustration may be pushed further by adapting from Lord Rayleigh;[†] the chance that an even division giving $n$ molecules in each half of the region may not be departed from by an excess of more than $r$ in one half, with an equal defect in the other half, is the value of the well-known function $2\pi^{-\frac{1}{2}}\int_0^\tau e^{-\tau}d\tau$, wherein $\tau = r/n^{\frac{1}{2}}$ which is supposed to be not very large. Thus there is an excess on one side exceeding $n^{\frac{1}{2}}$ molecules, during about one-sixth of the whole time; thus in 1 c.c. of gas at atmospheric pressure there is an excess of six thousand million molecules in one specified half of its volume, for one-sixth of the time, while in smaller volumes of gas the relative excess in a specified half rapidly becomes greater. In fact, as all depends only on $\tau$, the chances of a given percentage of departure from the mean diminish so as to be proportional to $n^{-\frac{1}{2}}$ when $n$ is increased.

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[*] Maxwell's 'Theory of Heat,' 1871, at the end.

[†] "On James Bernoulli's Theorem in Probabilities," 'Phil. Mag.,' 1889; 'Scientific Papers,' vol. 4, p. 375.

The short paper which has been described at length relates to the chance of uneven distribution of the molecules in space. But questions regarding the nature of temperature, according to Maxwell's illuminating conception for a gas, are concerned with the law of distribution of the velocities in the crowd of molecules. It does not involve a long step from Thomson's investigation of probabilities of spacial distribution to reach a new outlook into the inner molecular meaning of thermodynamics, for which science is indebted, in the first instance, to Boltzmann (1877).* The principle of Clausius that the trend of an isolated system is toward states for which the entropy continually increases may be put alongside the principle that the general trend of a system of molecules is through a succession of states whose intrinsic probability of occurrence—in the sense illustrated above—continually increases. The suggestion presents itself at once that this probability is in some way a measure of the entropy of the system, at any rate in systems of which we know enough to be able to calculate the probability. Boltzmann carried the calculation through, much on the lines above sketched from Thomson, for a mass of gas, arriving at the result that the entropy of any collocation is in fact proportional to the logarithm of the probability of its occurrence. This logarithm of the measure of probability satisfies the conditions characteristic of entropy: for instance, it is additive. But it probes deeper than formal thermodynamics can do: the latter is restricted to systems in which each infinitesimal element has a temperature, *i.e.*, has its own internal heat motions already averaged. The new interpretation of entropy is not subject to such restriction: it applies to systems whose elements of volume are in the inchoate condition existing before they have settled down to a temperature at all. In ordinary matter this settling down is nearly instantaneous; perhaps it is only in radiometer problems connected with rarefied gases that the state of matter in which it has not a temperature can concern us. But in the theory of the radiations pervading an adiabatic enclosure, the whole radiation does not settle down at all to a common temperature unless there is molecular matter present which can interchange it by the process of absorption and emission; in the absence of absorbing matter the energy corresponding to each wave-length retains its own temperature. Thus we can see how this wider probability-theory of entropy, which was first extended to radiation by Planck, is calculated to throw additional light, not derivable from formal thermodynamics, on the averaged constitution of the natural radiation which is in equilibrium of emission and absorption with molecular matter.

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* *Cf.* 'Vorlesungen über Gastheorie,' 1896, p. 42.

This digression into the most modern molecular theory has perhaps led us too far. The very interesting subject of the thermodynamics of radiation is only about twenty years old. Resting as it does fundamentally on the link with mechanical energy which is afforded by Maxwell's working pressure of radiation, Lord Kelvin would never admit its validity. The reason seems to be that he was never able to satisfy himself about any mechanical model of the relation of the atom to the æther that would give a mechanism for this pressural interaction between them. There are those who hold that the physical idea of an electron is sufficiently precise to make the *rationale* of light-pressure logical and secure. But Lord Kelvin would not consider it until he could visualise the whole process—see it in operation, as he used to say—to effect which completely would possibly go deeper than we may ever hope to penetrate; and this inability cut him off from what some consider to be the most refined and beautiful special development of the science which he founded.

The question naturally arises whether the establishment of the mathematical function that is fundamental for the theory of mechanical energy is not a subtler matter than this mere estimation of chances: in other modes of its investigation a powerful array of the dynamical properties of the medium is introduced. What becomes of them in the present aspect? The answer is that the chance cannot be estimated aright until we know all the conditions, dynamical and other, to which the distribution of molecules is subjected. The dynamical relations find their place as conditions restricting the possibilities of random distribution. If through ignorance some of them are overlooked, the chances will be in error; each new condition that is discovered modifies to some extent the whole process, and thus amends our knowledge.

But this aspect of entropy is quite in keeping with the subjective character, so to speak, of available energy. Objectively, the dissipation of energy is merely the progress towards an equilibrium. As regards the purposes of man, whole regions of available energy may exist, of which he is ignorant, because he does not happen to have learned how to use them. The amount of energy available at a given temperature in a lump of carbon is possibly not yet exactly known: the process of turning it into heat before utilising it of course wastes most of it. Once, however, any slow reversible method of combustion has been discovered, in a voltaic battery for instance, the determination will be possible and may be effected once for all. Or, following a hint thrown out by Lord Rayleigh in 1875, afterwards developed more fully by Gibbs, we may make a rough estimate by applying the Carnot-Clausius formula to a cycle of which the upper temperature is that of spontaneous dissociation of the materials. We can, in fact, ascertain available energies only for systems which we can reach from a standard one by processes reversible in Carnot's sense.

Very early in Joule's investigations (1841) on the quantitative equivalence of various kinds of energy, he attacked the problem of the voltaic cell, and found his expectation verified, that in many cases the electromotive force was proportional to the thermal value of the chemical action of one Faraday equivalent of the reagent materials,—provided he employed* "galvanic arrangements adapted to allow the combinations to take place without any evolution of heat in their own localities." He concluded that the condition thus laid down must be departed from in certain observed cases of discrepancy, and Thomson, in 1852,† conducted experiments to detect such local reversible heat. This principle of Joule was also stated quantitatively later, in a general way, by Helmholtz in the 'Erhaltung der Kraft' in 1847. It lies at the foundation of Thomson's memoir of December 1851, "On the Mechanical Theory of Electrolysis," whence the restriction above stated, the absence of local reversible heat, is quoted. *On this condition* the principle is exact; and the main point of Thomson's paper is the calculation, with a view to comparison with direct experiment, of the theoretical absolute value of the electromotive force of a Daniell's cell, from Joule's measurements of the heat developed by the combination of an electrochemical equivalent of its materials. The paper also developed the parallel between chemical energy and mechanical energy as sources of electromotive force, including the deduction by the principle of energy of the force induced by motion of a circuit across a permanent magnetic field. The further prosecution of the main subject, into cases where local reversible heat is developed (as evidenced by sensible change in electric conditions with temperature), remained for Gibbs and Helmholtz twenty-four years afterwards. In another paper of the same date, on absolute electric measurement, Thomson discusses Joule's thermal determination of absolute electric resistance of 1846, which afterwards proved to be more correct than the earlier values of the ohm.

Most interesting in connexion with modern ideas is an abstract of February 5, 1852,‡ again mainly expounding Joule's inspiring results and views on the transformations of energy. Thomson estimates from Liebig's data that about one-thousandth part of the total solar radiation incident on forest land is absorbed usefully by the trees, that being the amount recoverable as heat by their combustion. An intention to discuss these matters in connexion with Carnot's principle, dealing also with the wave-lengths of the radiation, does not appear to have been fulfilled. Passing on to animal work, he estimates, after Joule, that as much as one-sixth of the energy of the food consumed can go directly into mechanical power. Then, relying on Carnot's principle, and Joule's discoveries regarding the heat of electrolysis and of electromagnetism, he proceeds to argue that "it is nearly certain that when an animal works against resisting forces, there is not *a conversion of heat into external mechanical effect*, but the full thermal equivalent of the chemical force is *never produced*,—in other words, that the animal body does not act as a *thermodynamic engine*; and very probable that the chemical forces produce the external mechanical effects through electrical means."

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* 'Math. and Phys. Papers,' vol. i, p. 477.

† *Loc. cit.*, p. 503 : *cf.* also p. 496, where, in agreement with Joule, he ascribes the main loss to the work done by evolved gases in expanding against the atmospheric pressure.

‡ *Loc. cit.*, p. 505.

Here he is emerging from the narrower theory of heat to the general theory of available energy, where heat is not the intermediary towards mechanical power; and we shall see presently how quickly he progressed in it. When it is recalled that at the time all this was going on, or immediately after, he was also laying the dynamical foundations of the phenomena of induced electric currents, including, for example, the calculation of the period of the vibrations produced by electric discharges, the activity may well seem unprecedented; adequate exposition of the results had to fall behind.

The next stage (1855) in this series of investigations, the development of the ideas expressed in the extract just quoted, seems to demand special attention, for it is surely nothing less than the laying down of the precise laws of the all-embracing modern science of free or Available Energy. The evolution of this generalisation can, as it happens, be traced. The memoir on "A Mathematical Theory of Magnetism" has been already alluded to. In it, as everywhere else in Thomson's dynamical writings, the conservation of the potential energy, used there in the manner of Lagrange and Green and MacCullagh and Helmholtz, in the sense of a potential of mechanical forces, is employed to determine the essential relations between physical properties. This use of the law of energy as a connecting principle afterwards became the note of Thomson and Tait's 'Treatise on Natural Philosophy.' In revising for press a continuation of this magnetic memoir, 'Phil. Mag.,' April 1855, where he is engaged in deducing magnetic reciprocal relations in more elementary fashion by use of a work-cycle, a thought occurred to him and was embodied in a footnote under date March 26, which will be quoted in full.*

"It might be objected that perhaps the magnet, in the motion carried on as described, would absorb heat and convert it into mechanical effect, and therefore that there would be no absurdity in admitting the hypothesis of a continued development of energy. This objection, which has occurred to me since the present paper was written, is perfectly valid against the reason assigned in the text for rejecting that hypothesis; but the second law of the dynamical theory of heat (the principle discovered by Carnot and introduced by Clausius and myself into the dynamical theory, of which, after Joule's law, it completes the foundation) shows the true reason for rejecting it, and establishes the validity of the remainder of the reasoning in the text. In fact the only absurdity that would be involved in admitting the hypothesis that there is either more or less work spent in one part of the motion than lost in the other, would be the supposition that a thermo-dynamic engine could absorb heat from matter in its neighbourhood, and either convert it wholly into mechanical effect, or convert a part into mechanical effect and emit the remainder into a body at a higher temperature than that from which the supply is drawn. The investigation of a new branch of thermo-dynamics, which I intend shortly to communicate to the Royal Society of Edinburgh, shows that the magnet (if of magnetised steel) does really experience a cooling effect when its pole is carried from $A$ to $B$, and would experience a heating effect if carried in the reverse direction. But the same investigation also shows that the magnet must absorb just as much heat to keep up its temperature during the motion of its pole *with* the force, along $AB$, as it must emit to keep from rising in temperature when its pole is carried *against* the force, along $DC$."

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* 'Elec. and Mag.,' § 672. In a less definite way this principle had been effective long before, as the writer is reminded by Mach's historical account. Early in 1849 James Thomson explains that it was his brother's pointing out to him that, on Carnot's principle, water could be frozen isothermally without requiring mechanical work, which set him on to the train of thought that predicted the lowering of the freezing-point by pressure and calculated its amount. As freezing is accompanied by expansion, a cycle involving freezing at a high pressure and melting at a low pressure, in fact confronted him with a perpetual motion, which he had to evade.

The exposition of the new branch of thermodynamics here referred to appeared (*cf. supra*, p. xiv) in the same month, April 1855, in the first part of the first volume of the 'Quarterly Journal of Mathematics,' under the title "On the Thermoelastic and Thermomagnetic Properties of Matter, Part I," which represents the contents of the latter part of the paper, to which the more general introductory matter was probably added. This paper was reprinted in 'Phil. Mag.,' January 1878, with some additional notes.* The principles that we are now concerned with occupy the first few pages; the argument is expressed in terms of elastic strain, but that is obviously only for convenience of exposition. The total intrinsic energy $e$ of a material system, measured from a standard initial configuration and temperature, is defined as a function of its actual configuration and temperature. It is established from Carnot's principle, as in the quotation above, that for transformations conducted entirely at the same definite temperature $t$, the mechanical forces applied to the system must be derivable from a work function $w$ which represents, in fact, the potential energy acquired by the system in passing at that temperature from the standard configuration to the actual one. If $\epsilon$ denote the simultaneous increment of $e$, then $\epsilon - w$ must be the heat H taken in from outside during that change from the standard configuration, when conducted at the actual temperature.

It is to be observed that this simple consideration, which apparently here appears in science for the first time, carries the principle of potential energy in its mechanical application right back to Carnot's principle of 1824. In the previous writings on general potential energy, such as Helmholtz's 'Erhaltung der Kraft,' nothing of the kind is hinted at; while Clausius' treatment, being restricted to transformation of heat, is nowhere connected up with the general theory of energy. The first law of Thermodynamics henceforth drops to more restricted scope, for it merely asserts that available energy when lost is changed into heat in equivalent amount. Yet it still suffices to maintain the presumption that all energy-processes have their source in—are consistent with—the ordinary Newtonian principles of dynamics as applied to ultimate molecules; considering the difficulty experienced by Thomson in reconciling Joule's law with his innate conviction of the validity of Carnot's principle, it is not surprising that this inference appealed to him with special force. Indeed, when the historical conflict between the two laws is kept in mind, the value of the first will not be disparaged. From this point of view the principle of Carnot appears in transformed aspect. Its chief interest is now transferred to the two creative ideas which it contains, the introduction into science (i) of the idea of a complete cycle of transformations, and (ii) of the criterion of absence of waste of power in any mechanical process, namely, that the process can be reversed, which includes the condition of temperature uniform throughout the system at each instant. The further development, including Carnot's function and the quantitative determination of the idea of temperature which it brings with it, is the thermal completion of these fundamental principles of the general science of Energetics. When the illustrious originator of these ideas died in 1832 at the age of 36 he was in possession of the material to complete the train of essential principles himself.

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* 'Math. and Phys. Papers,' vol. 1, pp. 291-316.

Thus far we have secured a work-function $w$ (available energy) for the applied forces at each temperature $t$, of form determinable by direct experiment. If such a function were known for every temperature, knowledge of the mechanical energy relations of the system would be complete. Thomson accordingly proceeds to connect these functions for adjacent temperatures by means of a Carnot cycle. In fact, he shows how to construct $w$ as a function of both the configuration and the temperature, so that the same function shall, for each constant temperature, represent the energy then available for work.

The cycle which he employs is quite general, irrespective of the type of configuration for which it is conducted. In fact, consider any definite change of configuration effected at temperature $t$ and annulled at an infinitesimally near temperature $t+\delta t$, so that no work is done at the two infinitesimal transitions from the one temperature to the other, which complete the cycle. There will be heat H taken in at temperature $t$, and $\mathrm{H}+\delta\mathrm{H}$ given out at temperature $t+\delta t$, while there will be heat $\frac{\partial e}{\partial t}\,\delta t$ taken in at the upper transition, and $\frac{\partial e_0}{\partial t}\,\delta t$ given out at the lower, each of the latter heats being at an average temperature $t+\frac{1}{2}\delta t$; also if the lower configuration is taken to be the standard one $e-e_0=\epsilon$. Thus the equation $\Sigma\,(\mathrm{H}/t)=0$ of Carnot-Clausius leads to

$$\frac{\mathrm{H}}{t}-\frac{\mathrm{H}+\delta\mathrm{H}}{t+\delta t}+\frac{\partial\epsilon/\partial t\,.\,\delta t}{t+\frac{1}{2}t}=0,$$

which is, in its exact limiting form,

$$\frac{\partial\epsilon}{\partial t}=t\frac{\partial}{\partial t}\frac{\mathrm{H}}{t}. \qquad (5^*)$$

Substitution from $\epsilon=w+\mathrm{H}$ gives the equivalent forms

$$\frac{\mathrm{H}}{t}=-\frac{\partial w}{\partial t}, \qquad (6)$$

$$\epsilon=w-t\frac{\partial w}{\partial t}; \qquad (7)$$

so that, when the temperature changes as well as the configuration,

$$e=\int_{t_0}^{t}\kappa dt+w-t\frac{\partial w}{\partial t}, \qquad (8)$$

where $\kappa$ is the specific heat of the mass at $t$ when maintained in the standard configuration. This latter equation determines the total energy $e$ from the mechanical observations giving $w$, which is the work required to pass at temperature $t$ from the standard configuration to the actual one.

Conversely, by (5), for a particular configuration,

$$\mathrm{H}=t\int\frac{d\epsilon}{t}, \qquad (10)$$

and, for a constant temperature,

$$w=\epsilon-\mathrm{H}, \qquad (11)$$

"which show how H and $w$ may be determined for all temperatures from a knowledge of the intrinsic energy of the body, and of [one of] those functions themselves for a particular temperature."

The slight correction "[one of]" introduced in the Reprint (1882) is evidence of importance attached by Thomson to this investigation; yet it seems to have escaped general appreciation. On being asked about a year ago why he had been content with this brief, almost incidental, indication, and had never returned to the exposition of these fundamental quantitative relations of Available Energy, the letter in reply was simply to the effect "Yes; it is all there; there is nothing to be added."†

The two functions, total energy $e$, and work of available energy $w$, on which the complete science of Energy is thus founded, are naturally to be compared with the two functions, energy U and entropy S, which were made fundamental by Clausius in the very same month, April 1855—the tendency of the entropy *of a self-contained system* to increase being his mode of exact expression by Thomson's principle of dissipation. In fact, the distinction between the two methods is that Thomson's function $w$ refers primarily to a system fed with heat so as to remain at constant temperature, while Clausius' function S refers primarily to an isolated system.

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* The equations are numbered as in Thomson's paper of 1855; H is here supposed expressed in units of energy, so that the factor J is omitted.

† In a brief note "On Thermodynamics founded on Motivity and Energy," 'Roy. Soc. Edin., Proc.,' March 21, 1898, Lord Kelvin has himself recalled attention to the generality of this paper of 1855.

The principal operations of chemistry and physics are performed at constant temperature; thus it is Thomson's function $w$ that is fundamental in the modern science of Energy, having been reintroduced by Willard Gibbs as "the characteristic function at constant temperature," and by Helmholtz as "free energy." The entropy is simpler to describe, and also to work with, except when the operations are isothermal; on the other hand the "free energy" is a direct physical conception connecting up heat-energy in line with all other types of available physical energy, and thus transforming thermodynamics into the universal science of the relations of the statical transformations of Energy, namely, Energetics.

The function entropy seems to have been never employed in Lord Kelvin's investigations. As may be inferred from the above, it did not lie directly in his line of thought, which concerned itself with the physical entities energy and work. The idea of entropy is so directly suggested by his principle of dissipation, and the early mastery of the Carnot-Clausius equation $\int(d\mathrm{H}/\mathrm{T}), = 0$ for a reversible cycle in its widest form, which is shown in his theory of thermo-electric phenomena, that it could hardly have been strange to him; conceivably he never directly formulated it, because he had, in fact, developed a more directly physical scheme.

It is customary, after Thomson's own example, to call the relation $\int(d\mathrm{H}/\mathrm{T}) = 0$, as above, the Carnot-Clausius equation. It would provide the necessary complement to this nomenclature if the equation (7), that is, in more usual notation, the equation of energy A available at constant temperature T,

$$\mathrm{A} = \mathrm{E} + \mathrm{T}\frac{\partial \mathrm{A}}{\partial \mathrm{T}},$$

which is now the fundamental principle in chemical physics through the far-reaching applications made by Gibbs, Helmholtz, Van't Hoff, Nernst, and other investigators, were known as the Thomson equation. His dominating position is indeed already widely, but not very definitely, recognized.

The question whether Thomson had prior knowledge of the entropy principle has been matter of some controversy between Clausius and Tait: on the view here taken it is relatively unimportant.

We may now recall in general terms the form of the principle developed into most varied applications by Willard Gibbs, with such power and invention as to constitute him the creator of a new science. The necessary increase of the entropy function S defines the trend of adiabatic transformation; the necessary decrease of the available energy function A defines the trend of isothermal transformation.

The two functions are immediately connected by noticing that the S in the given configuration exceeds $S_0$, that in the standard configuration at the same temperature T, by $-\partial A/\partial T$. We can render an isothermal transformation adiabatic by including in the system an infinite reservoir of heat at its own temperature, in the manner favoured by Planck: the change of total entropy is that of $S-H/T$, so that this function must always increase in an isothermal system. The reverse transition from adiabatic to isothermal would not be so direct. In fact, the entropy S is the convenient analytical function to employ when the temperature is different in different parts of the system, as is illustrated by the complexity of the calculation (already conducted in February 1853, in terms of Carnot's function $\mu$) of the energy available for mechanical effect in such a system when self-contained,* which is mainly of cosmical interest, and has probably drawn attention away from the principles of free energy, though the latter were again emphasised in Thomson and Tait's 'Natural Philosophy.'

This analysis of available energy by Thomson had not escaped the notice of Willard Gibbs (1876), though possibly only in its narrower connexion with elasticity.† "Such a method is evidently preferable with regard to the directness with which the results are obtained. The method of this paper shows more distinctly the *rôle* of *energy* and *entropy* in the theory of equilibrium, and can be extended more naturally to those dynamical problems in which motions take place under the condition of constancy of entropy of the elements of a solid . . . just as the other method can be more naturally extended to dynamical problems in which the temperature is constant." Gibbs then refers back to a previous note explaining the wider generality of his own method: its most salient feature is, however, the far wider development, by its author, into the doctrine of the chemical potentials of the constituent substances.

As throwing light on the stage at which scientific thought had arrived at the time Thomson was thus formulating the general science of Energetics, the following quotation from Helmholtz's important lecture,‡ "On the Interaction of Natural Forces,"—delivered first at Königsberg, February 7, 1854, and in which he was the first to refer the replenishment of Solar heat to gravitational shrinkage,—is pertinent to our history. "These consequences of the law of Carnot are, of course, only valid provided that the law when sufficiently tested proves to be universally correct. In the meantime there is little prospect of the law being proved incorrect. At all events we must admire the sagacity of Thomson, who, in the letters of a long-known little mathematical formula which only speaks of the heat, volume, and pressure of bodies, was able to discern consequences which threatened the universe, though certainly after an infinite period of time, with eternal death."

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* Thomson, *loc. cit.*, p. 554. The calculation of the final uniform temperature is in fact based (p. 556) implicitly on constancy of the entropy.

† 'Scientific Papers of J. Willard Gibbs,' vol. 1, p. 204.

‡ English translation (by Tyndall), vol. 1, 1873, p. 172.

Later, in 1861, in writing of the constant surprises that arose in his work on acoustics, and the impression borne in upon him that new results develope themselves in the mind according to laws of their own, so that it seems to be hardly things essentially of his own invention that he is reporting, Helmholtz suggests that "Mr. Thomson must have found the same thing in his own work on the mechanical theory of heat."*

At the meeting of the British Association at Belfast, in 1874, Andrews seems to have communicated verbally his results on the critical temperatures of mixed gases—only published posthumously in 1886—including the fact that the presence of nitrogen increases the quantity of carbonic acid that will evaporate into a given space. These results strongly attracted the attention of Maxwell, as appears in letters to Andrews of date 1874 and 1876,† the earlier referring to his own recent construction of Gibbs' thermodynamic surface; but it appears most remarkably in a letter to Stokes of date August 3, 1875,‡ in which he spelled out the whole abstract theory of the conditions of co-existence of two phases in a mixture of substances, exactly in the manner of Gibbs, and, moreover, looked forward to getting clearer ideas regarding the functions afterwards named by Gibbs the 'potentials' of the constituents, by applying the method to simple systems. At that very time Gibbs was preparing for press the profound and exhaustive treatment, over the entire range of known phenomena, and even into others yet unrecognised, which has become a classic in scientific literature.

One other important landmark in the development of Thermodynamics into Energetics is instructive historically. As Helmholtz afterwards discovered, the importance of the principle of dissipation of energy as the true criterion of chemical reaction was enforced by Lord Rayleigh in a discourse at the Royal Institution in 1875,§ in which, naturally without mathematical development, he pointed out the bearings of the criterion on the problem of solution of salt in water, on the modification by pressure of the equilibrium between carbonic acid gas and chalk, and in imposing a limit with rise of temperature to the combination of oxygen and hydrogen. In the first quantitative investigation of this kind, communicated to the 'Philosophical Magazine' in the same year, he calculates how much energy is dissipated by the isothermal mixture of two different gases, on the basis of a result of the kinetic theory. The conclusion, a very simple one, which immediately attracted the attention of both Maxwell and Gibbs, is, as he explains, verifiable at once by taking advantage of the reversible mode of separation afforded by absorbing one of the gases either by solution or by chemical action: it thus forms a stage towards the more daring introduction by Gibbs of theoretical semi-permeable partitions of various kinds which has since become such a convenient and even practical feature in chemical physics.

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* 'Life,' p. 205.

† 'Scientific Papers of T. Andrews,' Introduction, p. liv.

‡ 'Memoir and Scientific Correspondence of Sir G. G. Stokes,' vol. 2, p. 34.

§ 'Scientific Papers,' vol. 1, p. 240.

Some confusion between the two main modes of development, that of entropy and that of available energy, to the detriment of the latter, was at one time accentuated by the misunderstanding* of Clausius' function contained in the earlier editions of Maxwell's 'Theory of Heat,' but corrected in the fourth edition (1875), where, however, he is still far behind Thomson's definite position of 1855, for he sums up with the remark (p. 193) that "The calculation of the energy dissipated during any process is therefore much more difficult than that of the increase of the total entropy."

If, however, the researches into the principles of available energy are, from an abstract and philosophic point of view, the most striking of Lord Kelvin's achievements, the practical side of his genius operated more persistently in other ways, for example, in connexion with the introduction and establishment of a scientific system of measurement of electrical quantities. Not only did he enlarge and enforce the advantages of a universal correlated system of units, such as had been developed in the narrower field of the distribution of terrestrial gravity and terrestrial magnetism by Gauss and Weber because in fact they were indispensable to international co-operation in these subjects: he was also the prime mover in starting those determinations of absolute constants of nature and of numerical relations between the various natural standards, which, repeated and refined by a long line of eminent successors, are now the special care of governments, as affording the universal data on which modern exact engineering is ultimately based.

One of the main incitements to this development of electrical science on an exact basis of practical measurement was doubtless provided by the problem of submarine telegraphic communication. The earliest successful cable between this country and the Continent dates only from 1851; and the phenomena which obstructed its speed of working, both the amount of electricity which it took up, owing to its large capacity, before sensible effect could be produced at the other end, and the soakage into the insulating material, had come under the consideration of Faraday. These difficulties, as well as the mechanical obstacles to laying it, would be far greater in a longer cable; but already in 1857 the funds were forthcoming, owing mainly to the zeal of Cyrus Field, and a cable was laid to America. Thomson gave the project his strongest support, even becoming a director of the company responsible for the enterprise. But the methods adapted to signalling through so long a cable had not yet been developed: and mistaken attempts, on the part of those in charge of it, to accelerate the speed of working by feeding it with electricity of high tension, led soon to rupture of its insulation. Next year another cable was laid down and was operated successfully for a few weeks before it developed a fault. Thomson had now been given a free hand, and all his force must have been concentrated into carrying through to success the costly enterprise for which he had made himself responsible. He was now able to put into operation his own ideas: instead of strength or tension of current, he relied on the other alternative, extreme delicacy of the receiving instruments. At first he utilised, both in 1858 and on the later cables of 1865 and 1866, the galvanometer, adapted into its most sensitive form, by developing the method of Gauss and Weber of observing very slight motions of the very minute needle, situated at the centre of a small coil at first arranged on the Gaugain-Helmholtz plan, by the reflexion of a beam of light from it—using thus a non-material pointer, as he expressed it, whose length was not subject to limitation from any difficulties relating to weakness or weight or inertia. In 1870 he replaced this mode of reading signals from the oscillations of a spot of light, by a method which actually wrote the message on paper—the famous siphon recorder still employed for long cables. The magnet of his galvanometer had to be very small in order to get it into the cavity of the coil through which the cable-current flowed, so as to be in the most intense field of force relative to its size; and there was of course a limit to the magnetism it could retain. It was thus out of the question to attach to it directly the inertia of any writing apparatus. Thomson therefore reversed the circumstances: it was now the coil that was free to swing, and when the undulations of current passed through it, dead-beat oscillation ensued arising from its being suspended in the narrow concentrated field of a powerful magnet. In this *heterostatic* system of receiving signals or measuring effects, as he had called it in his early electrometers (1857),* the mechanical force was capable of increase so long as the power of the accessory magnet could be increased: it was easy to get the magnet strong enough to permit the coil to carry a small siphon filled with ink, which spurted a permanent trace of its own transverse oscillations on an electrified sheet of paper moving lengthways beneath it.

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* Pointed out by Gibbs in 1873, 'Scientific Papers,' vol. 1, p. 52, footnote.

It may be noted here that in delicate galvanometry, after a long reign of the original Thomson type of instrument with reflecting magnetic needle, the construction has largely passed over to this heterostatic form, first made convenient for ordinary work by d'Arsonval by the use of a permanent steel magnet. In the earlier days some direct measure of the relative magnitudes of currents was oftener needed: but the progress of electric standardising, and the consequent development of exact methods of balancing against such standards, have now made measuring arrangements so handy and convenient that variation of the scale due to gradual changes in the steel magnet is of little account.

It was perhaps the same necessities, the insulation of cables, that led him into exact investigation of the magnitude of the electric forces that would throw sparks across a given breadth of air or other material; though the development of the electrometers suitable for this purpose had begun with him very early in his career. His investigation of the attraction of two mutually influencing spheres had this object in view (*supra*, p. x); and he has put on record how, about the same time, 1846 or 1847, he was impressed by finding in the Cavendish manuscripts, then in the hands of Sir W. Snow Harris at Plymouth, his marvellous experimental determination of the capacity of a circular disc as compared with a sphere. An immediate application of the electrometer, as thus rendered absolute, was to the exact measurement of the voltaic effect of contact between different metals: it was found that connexion through a drop of electrolyte annulled the effect, and the meaning of this and related observations remained for long a matter of controversy, perhaps not yet settled. The continued improvement of standard electrometers, as distinct from mere electroscopes, was also stimulated by his interest in the problem of atmospheric electricity, in which he followed up the work of Beccaria. He was naturally an admirer of the science of the Earth's magnetism as it had been securely founded by Gauss and Weber: he seems to have conceived the aim, by the use of similar methods, namely, the steady collection and discussion of exact observations, of establishing a science of the Earth's electricity. The conditions were naturally far more complex: the wide and erratic fluctuations in an unstable meteorological phenomenon, such as the electricity of the atmosphere, were very intractable, in comparison with the more steady secular modes of change of the magnetic field of the Earth. Though even yet knowledge of the genesis of thunderstorms is very far from definite, the foundations of what is known are based largely on Thomson's pioneering investigations.

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* 'Elec. and Mag.,' pp. 262, 310.

But no increase of mere instrumental sensitiveness could have availed to increase the speed of signalling in cables. The problem of how to obviate the deleterious diffusive effects due to electric capacity of the cable was one for mathematical study, on the methods of Thomson's own earliest mathematical writings, those of the Fourier Theory of Diffusion of Heat; for the electric impulse merely diffuses along such a cable, like heat along a bar, instead of being propagated by definite waves. Already in 1854 the outlines of the theory of the relation of length of cable to speed had been worked out in a correspondence,—apparently stimulated by Stokes, though he was always very generous in his acknowledgments,—which had only to be further developed in order to find the kind of compound electric impulses communicated at one end which would give quickest and most definite observable response at the other end. Throughout 1856 he insisted on the correctness of his principles, which were indeed mathematically irrefragable, in the 'Athenæum,' the 'Proceedings of the Royal Society,' and elsewhere, and as we have seen he was ultimately authorised to put them into practice.*

At this time, too, he just touched on the other type of action, that of electrodynamic induction,† in the confusing of the propagation of signals, pointing out that in ordinary telegraphy the alternations did not follow one another with rapidity sufficient to render this effect of practical importance. But with the invention of the telephone, transmitting the far more rapid vibrations of speech, it soon came to the front. Long afterwards (1889) in lecturing with keen appreciation on Heaviside's mathematical prescription, at first sight paradoxical, for removing the difficulties of long-distance telephony simply by interposing suitable inductance coils in the circuit, he recurs to some of his early experiences afloat with his associates the Atlantic cable engineers, recounting how they then knew from experience, and understood by reason, that even leakage was a good thing for cable signalling, provided they could have it where it was wanted without the risk of having too much of it.

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* *Cf.* 'Math. and Phys. Papers,' vol. 2, pp. 60—111.

† *Cf.* also 'Baltimore Lectures,' Appendix L.

Thus from about 1857 a main portion of Thomson's energies became diverted into other channels. The wonderful flow of new scientific principles, of permanent interest for all time, which was characteristic of the preceding twelve years, and is represented roughly by the first volume of his Collected Papers, is now largely suspended; his main activity is devoted to the (in some respects) more ephemeral, but equally valuable, aim of rendering available by mechanical appliances, for the purposes of practical life, the knowledge thus acquired. But when the troubles with the cables had been finally surmounted in 1867, by an experience which had made Thomson a resourceful engineer as well as a physicist, a new outburst of theoretical activity arrived.

Among the most potent causes of the general improvement in physical modes of thought during the last third of the century, was the appearance, in 1867, of what then purported to be merely the first volume of the 'Treatise on Natural Philosophy' by W. Thomson and P. G. Tait, which has proved to be a turning point in the exposition and expression of physical science, at any rate in this country. The preparation of this book, which had gone on for some years, induced frequent visits by Thomson to his friend and disciple Tait at Edinburgh. Among other things, this treatise revised the terminology of dynamics, which had been allowed to grow up, in many respects, in forms that retained only historical meaning; the impulse thus given, which had indeed already been operating less systematically in the previous years, and was largely due doubtless to his brother James Thomson, has led in the hands of Maxwell, Heaviside, and others elsewhere, to greater attention to the language of science, the introduction everywhere of expressive terms, which react powerfully in inducing clearness of ideas. Another of the benefits conferred by this work was that it served, in some degree, to focus the scattered fragments of Thomson's own investigations and those of his associates, and to exhibit his scientific method, as exemplified in the subjects covered in this first instalment, which contained general kinematics and dynamics, general theory of the potential, and theory of elasticity with extensive geodetic application.

A translation of this book into German, by Helmholtz and Wertheim, appeared in 1871–4. In a preface, Helmholtz pointed out how it satisfied, in very remarkable manner, a most urgent want in higher scientific literature. Previously there had been no resource but to go to original memoirs, difficult of access even if one knew where to find them; and on this account the recent progress of connected mathematical physical thought had been halting. Moreover, as he said, when a worker like Sir William Thomson admits us to participate in the very upbuilding of his ideas, exhibits to us the modes of intuition, the guiding threads, which have helped him, by bold combinations of thought, to control and arrange his refractory and entangled materials, the world owes him its highest gratitude. Helmholtz goes on to contrast the universal outlook of such a book, involving unavoidable lacunæ and difficult transitions, with the beautiful precision of the best special treatises of the earlier period. But the reader who does not spare himself the necessary effort towards mastery reaps an ample reward; he will find himself trained and equipped for the task of appreciating and extending knowledge, to a degree that he could never have attained from mere passive assimilation of sharply cut formal demonstrations. Valuable to the same end is the constant endeavour of such a work to employ those mathematical methods that keep close to actuality, are amenable to detailed interpretation; though they are usually much harder, especially at first, than a strictly ordered analytical calculus would be, there remains the permanent gain of direct insight into the processes and relations of nature. Finally, allusion is made to difficulties encountered by the translators, arising from the originality of the treatment, and the series of new scientific terms that the authors had, in consequence, introduced.

This appreciation, by the most competent living master, set out justly the advantages and defects of Thomson's method of work. He never had time to prepare complete formal memoirs. It was but rarely that his expositions were calculated to satisfy a reader whose interests were mainly logical; though they were almost always adapted to stimulate the scientific discontent and the further inquiry of students trained towards fresh outlook on the complex problem of reality, rather than to logical refinement and precision in knowledge already ascertained. Each step gained was thus a stimulus to further effort. This fluent character, and want of definite focus, has been a great obstacle to the appreciation of 'Thomson and Tait,' as it is still to Maxwell's 'Electricity,' for such readers as ask for demonstration, but find only suggestion and exploration. There is perhaps nothing that would contribute more at present to progress in physical thought than a reversion, partial at any rate, from the sharp limitation and rigour of some modern expositions to the healthy atmosphere of enticing vistas which usually pervades the work of the leaders in physical discovery. With increased attention to the inspired original sources of knowledge the functions of a teacher would be more than ever necessary, to point to the paths of progress and to contrast the effectiveness of different routes, as well as to restore valuable aspects which drop away in formal abstracts; science would thus adhere to the form of a body of improving doctrine rather than a collection of complete facts.

Readers of the 'Life of Helmholtz' will recognise how fruitful for knowledge was the intimate lifelong friendship between these two greatest investigators of the age. The 'Erhaltung der Kraft' of 1847 became known to Thomson about five years later, and he is insistent on the benefit conferred on British science by the appearance of a translation some years after in Taylor's 'Scientific Memoirs.' It was in August 1858, about the time Thomson's invention was perhaps at its highest development, that they first met. Thomson had written from Kreuznach urging Helmholtz to attend the British Association in September; he remarked that his presence would be one of the most interesting events of the meeting, and added the plea that he looked forward with the greatest pleasure to an opportunity for making his acquaintance such as he had desired ever since the 'Erhaltung der Kraft' had come into his hands. A few days later Helmholtz called on him, and reports as follows to his wife*:—

"I expected to find the man, who is one of the first mathematical physicists in Europe, somewhat older than myself, and was not a little astonished when a very juvenile and exceedingly fair youth, who looked quite girlish, came forward. He had taken a room for me close by, and made me fetch my things from the hotel, and put up there. He is at Kreuznach for his wife's health. She appeared for a short time in the evening, and is a charming and intellectual lady, but in very bad health. He far exceeds all the great men of science with whom I have made personal acquaintance, in intelligence and lucidity and mobility of thought, so that I feel quite wooden beside him sometimes. As we did not get through nearly all we wanted to say yesterday, I hope you will let me stay over to-day at Kreuznach."

A record of the personal impression produced at this period, when he was not much known in London, on so close an observer of character as Thackeray, occurs in the recently published correspondence of the essayist Dr. John Brown of Edinburgh; it will supplement the above more scientific appreciation. Thackeray had been making one of the earliest of his lecturing tours, and had gone on from Glasgow to Edinburgh. Dr. Brown writes in humorous vein to Lord Kelvin's relative, Miss Crum, on November 11, 1856. ". . . He (Thackeray) was delighted with your William Thomson; he said he was an angel and better, and must have wings under his flannel waistcoat. I said he had, for I had seen them . . ." In later years Dr. Brown seems to have made a point of sending to his friend reports of Lord Kelvin's public appearances.

It is to be remembered that Helmholtz's early professorial work was physiology, which it continued to be until 1857, when as a by-product of his acoustical researches the memoir on Vortex Motion appeared, one of the most brilliant results of mathematical genius of all time. It seems to have been about ten years later that Thomson's attention was definitely arrested by this memoir; on watching Tait experiment on the rebound of smoke rings in air after collision, the theoretical illustration of material atoms which is afforded by vortex rings in a perfect fluid medium is said to have leaped into view. Great mathematical activity at once ensued. The fundamental memoir of Helmholtz reappeared translated by Tait in 1867, and Thomson's beautiful development of vortex theory, in which the principles, proved by the rigorous abstract analysis of Helmholtz, were worn down to the current coin of general ideas relating to circulation and vorticity and cyclic motions generally, appeared in 'Trans. R. S. Edin.' (pp. 217–260) in 1867–8. So great was the insight into the underlying world of individual molecules that Thomson thought might arise from the development of this vortex analogy, that at one period he is said to have grudged all the time that was not devoted to its study. His papers and miscellaneous notes on vortex motion have not yet been collected; they contain many *tours de force* and some of his very hardest thinking. Nowadays nobody imagines that the molecules of matter are merely vortex rings, and it is doubtful whether Thomson ever thought they were so in any strict literal sense; but his language was certainly calculated to leave that impression, and for years the fascination of that view prevailed. What is equally certain is that the vortex theory has been a beacon light in the *arcana* of the physics of molecules; such other illustrative theories as have been helpful in this direction have been its lineal descendants, so that points of view now stand out as legitimate and illuminating which before the days of the vortex theory could hardly have been conceived precisely at all.

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* 'Life,' by Koenigsberger, Miss Welby's translation, p. 145.

At Easter, 1864, Helmholtz paid a visit to the Thomsons at Glasgow, with impressions again recorded in a letter to his wife.* "I have seen a quantity of new and most ingenious apparatus and experiments, which have made the two days very interesting. He thinks so rapidly, however, that one has to get at the necessary information about the make of the instruments, etc., by a long string of questions, which he shies at. How his students understand him, without keeping him as strictly to the subject as I ventured to do, is a puzzle to me; still there were numbers of students in the laboratory, hard at work, and apparently quite understanding what they were about."

The recreation of yachting, by which Thomson was wont to recruit his energies in summer, reacted naturally towards the improvement of nautical affairs. His dynamical instinct, and experience in the invention of delicate instruments, found a congenial field in placing the ship's compass on a scientific basis. The heavy cumbrous magnets swinging on pivots under unsuitable conditions were replaced by the well-known systems of needles, delicately suspended yet insensitive to shock, so small that the iron masses compensating for the magnetism of the ship could be effectively introduced in moderate size. Again, by the use of steel wire he worked up the modern method of taking reliable soundings from a ship in motion, the depth being calculated from the compression of the air in a narrow glass tube attached to the sinker. But the most remarkable feat in this domain was the thorough practical mastery of the complicated phenomena of the tides, achieved mainly under his direction, and culminating in the invention about 1876 of simple automatic mechanism for performing all the laborious calculations of tidal harmonic analysis, both direct and inverse. The tides are controlled by the Sun and Moon, and so repeat themselves very closely in periods of nineteen years. But there is another far more fundamental and instructive way of investigating them. To every periodic (simple harmonic) component in the motion of either Sun or Moon relative to the Earth, there corresponds a component of the same periodic time in the tide produced by them at any place, and there are no other components; yet to calculate their amounts directly with the existing irregular contours and depths of the ocean would be a problem practically impossible. The method of harmonic analysis, as first initiated in this subject on a much smaller scale by Laplace, allows us to deduce, from a tidal record for a sufficient length of time, the amplitudes and phases of these harmonic components of known periods; and when the more important ones have been thus determined, the prediction of future tides becomes a matter of merely summing up the harmonic constituents, no matter how complex the physical conditions at the place in question may be, so long as they are unchanging. All this and much more can now be done by the machines invented by Lord Kelvin and his brother,* though owing to the preliminary imperfection of construction of the analysing machine it is at present found to be safer and not very troublesome to determine the amplitudes of the components by calculation. This achievement—the complete mastery of the tides by means most simple but adequate—is perhaps the greatest triumph of the method of Fourier, which has always been one of the advances most admired by Lord Kelvin in modern physical mathematics. After this success it was natural to apply the same method of harmonic analysis to meteorological phenomena, including the atmospheric electricity which he had investigated many years before, which also are controlled by Solar influence; but here the problem has proved not to be so feasible, the definite periodic components being so mixed up with the erratic results of meteorological instabilities that not much has yet come out of the effort.

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* 'Life,' *loc. cit.*, p. 233.

In later years Helmholtz paid many holiday visits at Largs and enjoyed the yachting expeditions, which provided a refuge for him from the attacks of hay fever. In 1871 the two friends studied the theory of waves which Thomson "loved to treat as a kind of race between us." It was shortly before that Thomson had broken new ground suggested by observations from his becalmed yacht, on the theory of capillary ripples, and on the waves produced by wind and current, treated in two letters to Tait intended for the Royal Society of Edinburgh. In later years the latter subject was discussed in much more detail and developed in new directions by Helmholtz, with a view to meteorological atmospheric applications.†

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* See Thomson and Tait's 'Nat. Phil.,' ed. 2, Appendices.

† *Cf.* 'Baltimore Lectures,' Appendix G, and Prof. Lamb's 'Hydrodynamics.'

On board the yacht Helmholtz reports* that "It was all very friendly and unconstrained. Thomson presumed so much on his intimacy with them that he always carried his mathematical notebook about with him, and would begin to calculate in the midst of the company if anything occurred to him, which was treated with a certain awe by the party. How would it be, if I accustomed the Berliners to the same proceeding? . . ."

The subject of fluid dynamics in all its branches had always great attractions for Lord Kelvin, doubtless in part owing to his association with the naval architects of the Clyde during the modern evolution of ship-building; applications to the resistance to the motion of ships, the phenomena of waves and wakes, propulsion by screws, and such like, were never far from his thoughts. Even in his latest years he continued to write abstruse mathematical papers on theoretical hydrodynamics with an energy and facility which were the wonder of younger men.

It was in the prosecution of this subject of the reactions of moving liquid on immersed solids, that one of his most daring developments arose, such as perhaps would hardly have occurred to a mathematician more circumspect about his formal logic. This was the application straight off, in Thomson and Tait (1867), of the generalised Lagrangian dynamical method to the dynamics of solids immersed in liquid, which proceeded by entirely ignoring the liquid once the formula for the total energy had been determined in terms of the motion of the solids. It seems that Lord Rayleigh, and afterwards Boltzmann, had early called his attention to the need for verification of this procedure; and in the German translation (1871, p. 294) a proof by means of the Principle of Least Action, on lines that had been suggested by Boltzmann, is supplied by Thomson himself. Kirchhoff had already dealt with an extension of one of Thomson's special problems in illustration, in a somewhat similar manner in 1869.† In a footnote Thomson promises with characteristic confidence that the second volume of the 'Treatise' (which was never prepared) will contain a complete discussion, on the basis of Hamilton's principle, of the dynamics of cyclic motion. Something of this appeared later in the 'Phil. Mag.'

A thoroughgoing treatment ultimately came in the second edition of Thomson and Tait in 1879, but the foundation on Hamilton's principle was dropped. The theory of the elimination of co-ordinates, such as those of the individual particles of the fluid—Ignoration of Co-ordinates, as he called it—

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* 'Life,' p. 287.

† 'Abhandlungen,' p. 176. But in a paper on "The Motion of Free Solids through a Liquid," 'Proc. R. S. Edin.,' 1870–1, reprinted as an Appendix to 'Baltimore Lectures,' 1904, Thomson starts off by quoting from his private journal, of date January 6, 1858, the equations of 'Eulerian' type of the motion of a solid in liquid, expressed by the principles of momentum in terms of the six components of the translational and rotational impulse of the motion, which are themselves given as gradients of the function expressing the kinetic energy. This idea of 'impulse' was developed formally in the memoir on Vortex Motion of 1868. *Cf.* Prof. Lamb's 'Hydrodynamics.'

opened up, when thus generalised, an entirely new domain in the application of dynamics to general physics. In the ordinary Lagrangian dynamics the masses of which it treats are characterised by configuration and inertia; on the wider theory, they may also possess permanent momenta of spinning or other cyclic motions. By introducing this third endowment the horizon of physical application and dynamical elucidation is obviously very widely extended. The theory had been already published independently by Routh in 1877, in his Adams Prize Essay on 'Stability of Motion,' in more perfect form than Thomson's; for he had reduced the analysis into dependence on a single function, which he called the modified Lagrangian function of the system. The translation of the general theory, as thus compactly expressed, into relation with the principle of action and general Hamiltonian dynamics, was not difficult.*

At a later date (March 1884) Helmholtz got into the same subject, the modified Lagrangian function, in a series of papers on the Statics of Monocyclic and Polycyclic Systems, with application primarily to concealed cyclic motions such as might illustrate one possible aspect of the latent thermal energy in thermodynamics, or perhaps rather of the intra-molecular part of it. His treatment, which was identical with Routh's, culminated in a beautiful memoir in 'Crelle's Journal' (1886) on the physical significance of the principle of Least Action, in the course of which the idea of reciprocal theorems introduced by him long before in connection with acoustical problems, and developed in other directions more particularly by Lord Rayleigh and Maxwell, was placed on the widest foundation, in a manner which, however, was familiar to Hamilton himself in his own narrower physical range.

Another classical problem in mathematical physics in which new developments were included in the second edition (1883) of Thomson and Tait was that of the forms of a steadily rotating mass of gravitating fluid. Thus § 778 begins: "During the fifteen years which have passed since the publication of our first edition we have never abandoned the problem of the equilibrium of a finite mass of rotating incompressible liquid. Year after year, questions of the multiplicity of possible figures of equilibrium have been almost incessantly before us, and yet it is only now, under the compulsion of finishing this second edition of the second part of our first volume, with hope for a second volume abandoned, that we have succeeded in finding anything approaching full light on the subject." Then follow eleven propositions, stated without proof, arranging in most ingenious manner the known forms, so as to trace the order of transition between the successive configurations of stable and unstable steady motion, as the circumstances are gradually changed. In fact the problem was left ready for its next stage of development which came two years later (1885) in Poincaré's classical memoir, in which, building on the stationary property of the modified Lagrangian function or kinetic potential, the transitions between stabilities and instabilities in steady motion were made amenable to processes of continuous graphical representation. For the subsequent developments by Sir G. H. Darwin and others, with important bearings on problems of cosmical evolution, reference may be made to the last chapter of Prof. Lamb's 'Hydrodynamics.'

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* *Cf.* 'Proc. London Math. Soc.,' March 1884.

The evolutionary and tidal problems treated near the end of the original edition had in fact meanwhile been made his own by G. H. Darwin, so that it was natural that his assistance should be sought to bring the new edition up to the existing state of knowledge. In particular in an appendix, Part II, pp. 505–17, his beautiful graphical discussion of the secular effects of tidal friction is reproduced. The problem as to whether the waste of energy by terrestrial tidal friction came from the Earth or the Moon had been given up long before by Airy as intractable, a decision which, however, stimulated J. Purser, a close friend of Lord Kelvin and of his brother, to its definite and concise solution by combining the necessary conservation of the angular momentum with the frictional diminution of the mechanical energy.* The graphical treatment of the subject on the basis of these two relations had it seems been suggested by Lord Kelvin, with results which in Darwin's hands (1879) originated a new branch of astronomy, the dynamical theory of the evolution of planetary systems.

This discussion of cyclic systems, when energy is dissipated through friction, led† also to the fundamental distinction between ordinary and secular stability. "The gyrostatic forces which we now proceed to consider may convert instability into stability as in the gyrostat [with two degrees of freedom] *when there is no dissipativity;* but when there is any dissipativity, gyroscopic forces may convert rapid falling away from an unstable configuration into falling by (as it were) exceedingly gradual spirals, but they cannot convert instability into stability."

It would seem that one of the main endowments that go to the making of a mechanical engineer is an acquired sense of the inertia of matter, an instinctive feeling of what may be expected of great moving masses and the limits within which they may be controlled, whether they are ships straining on their cables, or fly-wheels whose normal function is in steadying rather than disturbing motion. This gift, which differs from the faculty of formal dynamical calculation as the instinct of a homing pigeon differs from the trials of a wanderer finding his way with a compass, was possessed in supreme degree by Lord Kelvin. It seems, in fact, to have been reserved for him to invent a distinctive name for the principle of rotatory momentum, of which the beginnings were already known to Newton though the bearings of the constancy of rotatory momentum in a self-contained system gradually came out into clearer light in the hands of his successors. It was expounded by d'Alembert how the spin of the Earth on its axis kept the direction of that axis fixed, except as regards the precessional motion definitely due to the torque of solar and lunar attraction on the equatorial protuberant matter—just as the overhanging weight of a spinning top produces precession of its axis round the vertical. The famous corollary seemed comparatively new when Laplace developed it, that it is possible to determine a plane of reference—his invariable plane, so called—in the Solar system, which must remain absolutely fixed in direction throughout all time, that namely of the resultant rotatory momentum. But instead of rotatory momentum these writers were accustomed to speak of rate of description of areas, in extension of the language of Kepler and Newton which was appropriate enough to a single planet travelling round the sun. When, however, we pass from the discrete planets of astronomy to the continuous spinning solids of dynamics, this terminology is nearly as unwieldy as the mathematical formulas themselves, and, worst fault of all, is quite unsuggestive. Though Poinsot's reform of procedure in statics, by introducing the idea of a turning couple or torque alongside that of a force, had undoubtedly much to do with the result, the dynamics of rotation began to assume a far more tractable form when expressed in terms of the incisive language developed in 1867 in Thomson and Tait's 'Natural Philosophy.' The remark of Helmholtz on the difficulty of translation of the new terms in the German edition may be recalled.

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* 'Brit. Assoc. Report,' Belfast, 1874, pp. 22–24.

† Thomson and Tait, Edit. 2, § $345^{vi}$; *cf.* also Lamb's 'Hydrodynamics,' § 197.

This dynamical instinct was not content to rest with a reconstitution in more formal terms of the principles of rotatory motion. To cultivate dynamical ideas further by actual acquaintance with the rotatory inertia of spinning masses, Thomson converted Foucault's gyroscope into a gyrostat, which may be considered as a fly-wheel with rapid spin impressed on it, isolated and protected inside a frame or case so that it could be manipulated in many ways, while this rotatory momentum would in the absence of friction remain a permanent static possession, effectively of the very essence of the gyrostatic system. The stiffness as regards direction, as shown by the wriggling resistance that such a body opposes to merely altering its orientation in space, opened up a new and fascinating domain in dynamics. In the second edition of Thomson and Tait (1881, p. 396), a section is introduced, with diagrams, illustrating some of the extraordinary ways in which these gyrostats can stand, balanced on an edge and in other strange positions, and refuse to stand in positions which are easily assumed by masses devoid of the concealed rotatory momentum. Such phenomena were to acquire fundamental philosophical significance in more than one direction, including the introduction of permanent latent motions into general dynamical theory referred to above, and the gyrostatic model of an optical æther which will be described presently.

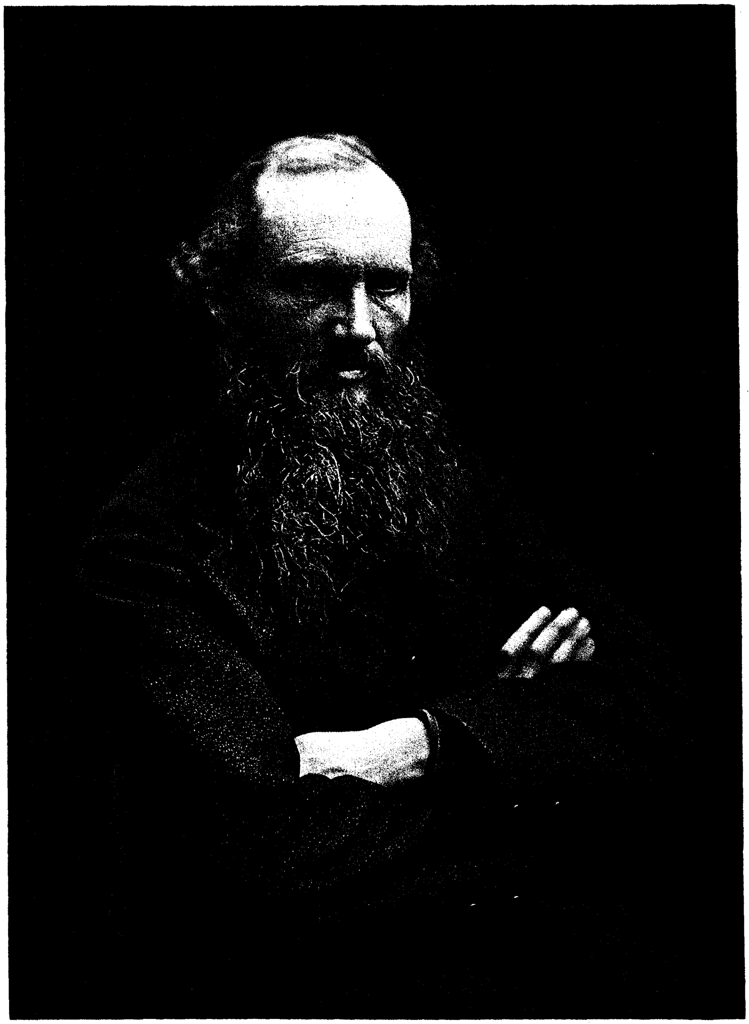
As experimental illustrations of the perverse effects of internal spin, the gyrostats were already of long standing, and their mysterious behaviour constituted one of the puzzles that greeted visitors to the laboratory at Glasgow. Already during his visit at Easter 1864, Helmholtz reports home to his wife an experience which illustrates another advantage of the enclosing case. "Thomson's experiments, however, did for my new hat. He had thrown a heavy metal disc into very rapid rotation, and it was revolving on a point. In order to show me how rigid it became by its rotation, he hit it with an iron hammer, but the disc resented this, and it flew off in one direction and the iron foot on which it was revolving in another, carrying my hat away with it and ripping it up."

In 1865, the principles of clock escapements and compensations began to occupy Thomson's attention, and after many efforts he reported to the Royal Society in 1869 on a standard astronomical clock which he had constructed, with strict attention to dynamical principles, and erected on a firm foundation in the hall of his house at the University of Glasgow, where it still remains. It does not appear that the promised future reports on the performance of this type of clock as compared with the usual observatory clocks were ever made. But long subsequently, in an address delivered at Manchester on watches and chronometers, he adverted, with some dissatisfaction, to the fact that for the customary expenditure of a few shillings, one could only procure a watch which with ordinary rough treatment would keep time to one part in ten thousand, or perhaps, with care, one in a hundred thousand.

He had communicated to the Royal Society in 1856 a formal memoir on another subject which occupied much of his later thought, the Mathematical Theory of Elasticity; in this paper the kinematic analysis is of very general type; but doubtless one of the aims was to reform and purify the theory by basing the subject on the appropriate thermodynamic function, the free or Available Energy, which had been established by him the year before (*supra*, p. xlvii), as the proper physical expression of Green's mathematical principle. This was followed in 1863 by a mathematical application, difficult for that time, to the stresses excited by rotation in elastic spheroids and in shells containing incompressible perfect liquid, with a view to the bearing of the results on the question of the rigidity and internal constitution of the Earth as tested by the amount of the astronomical precession and nutations. This application belonged to the geological and evolutionary part of his work, and also included considerations relating to shift of the Earth's axis, and the change of the length of the day owing to tidal and other causes; it was reprinted in 1878, but with corrections arising from the criticism of Newcomb and others, who had persuaded the author that he had actually himself underestimated the directional persistance of the rotating fluid interior.

In a Presidential Address to Section A of the British Association, Glasgow, 1876, an account was given of this pre-occupation with the Earth's rotation and rigidity, precession and nutation, tides and monsoons, meltings of polar ice, etc. The opening paragraphs of this Address express his vivid impression of all that he had just seen in a visit to the Philadelphia Exhibition (followed in later years by numerous other Transatlantic visits) of the great undertakings in organised scientific work that were being carried through in America. He includes a description of his experience with the telephone, then nascent, which excited his admiration (as it did that of Helmholtz) as a practical and definite embodiment of the fundamental doctrine which was illustrated in another phase by his own tidal developments—the Fourier harmonic analysis of oscillatory motion.

Since his return from America the criticism of Newcomb, above mentioned, had focussed his thoughts. As he explained in the Address, he had not yet begun in 1862 to ponder over the stabilities of vortices and other configurations of spin. The state of the case regarding terrestrial physics, as now amended, is shortly this:—"The hypothesis of a perfectly rigid crust containing liquid violates physics by assuming preternaturally rigid matter, and violates dynamical astronomy in the solar semi-annual and lunar fortnightly nutations; but tidal theory has nothing to say against it. On the other hand, the tides decide against any crust flexible enough to perform the nutations correctly with a liquid interior, or as flexible as the crust must be, unless of preternaturally rigid matter." The results of the mathematical investigation on the precession of a hollow spheroid filled with liquid, which thus restricted the argument to rapid nutations, were given in the Address, but his analysis has apparently never been published, though other investigators have since supplied the want. Passing on to the free wanderings of the terrestrial pole and the consequent slight changes in latitude, he insists that notwithstanding that Peters and Maxwell and others could find at that early time no periodic effect, yet the irregular changes that were noted must be in part real, because existing meteorological causes such as winds, the melting of polar ice, etc., are competent to produce displacement of from one-twentieth to one-half of a second of arc. He goes on to consider the tides resulting from such a displacement of the Earth's axis, which are quite sensible; in fact, their periodic components have since been sought for in the tidal records. He decides against the occurrence in past time of vast 'four hundred metre' tides recurring in three or four hundred days, such as would be started by a geological convulsion involving sudden elevation of one octant of the Earth by about that amount with subsidence of other regions; but he then saw no reason why the polar axis should not have gradually travelled in the course of ages, in close company with the axis of inertia, through many degrees to its present position. This remarkable Address closes with a consideration of the causes of a possible diminution of the Earth's period of rotation such as Lunar Theory seemed to demand; it was some time later that he detected unexpectedly a quite sensible thermodynamic acceleration due to the diurnal lag of the barometer behind the Sun. But in any brief abstract it is quite impossible to summarise these activities. The Report of this Meeting (1876) records merely the title of a paper 'Physical Explanation of the Mackerel Sky,' which went on lines now familiar in meteorology, namely, interfacial wave-action due to cross currents rubbing over one another. He also reported again on his astronomical clock (*supra*, p. lxiii) still with hope, but results had been delayed by unfortunate choice of materials and construction for



Yours very truly
William Thomson

1877

the compensated pendulums. At this very brilliant meeting of the British Association he read no less than eleven papers on the most varied subjects, and a long analytical report on tidal observations (pp. 275–307), not to mention other papers and reports partly inspired by him, in addition to the Presidential Address above quoted.

One of his striking mathematical achievements of a later date (the vortex motion period) was the demonstration that when a hollow ellipsoid of revolution filled with perfect liquid is set into bodily rotation, the motion of the liquid is stable only when the cavity is of the oblate kind. In illustration of this prediction he had hollow gyrostats made, which by aid of some skill could be set spinning bodily along with their liquid contents; the oblate one behaves as an ordinary gyrostat, but the prolate one, as soon as the orientation or the steady maintaining torque of the spinning cord is altered, makes a few violent wriggles and subsides by turbulent break up of the rotation of the liquid, which occurs the more rapidly the greater the spin it has acquired.

In the later years, in pursuance of elastic theory, much attention was devoted to the early view of optical double refraction and reflexion which ascribes to the æther the vibratory properties of a solid body. These properties were exactly specifiable as above by the function representing available energy, in the manner first employed by Green, to whom was due the first definite idea that the energy function provided the criterion of the precise amount of complexity that it was permissible to introduce in continuous elastic phenomena. For many years Thomson struggled hard, in successive papers full of brilliant subsidiary results, to bend somehow the elastic solid theory so that it might be forced into compliance with the very various conditions imposed by the optics of both isotropic and crystalline media. Sometimes he seemed to come near to success. But success was achieved only when he cast aside the idea of solid elasticity, and betook himself to the help of the anomalous elastic reaction belonging to his gyrostats, in the form of resistance to rotation rather than deformation. But it is of no use to this end merely to build up a model of a medium filled with spinning flywheels; the reaction to rotation would then be too complex. Each such flywheel is dynamically a directed element; if their axes are distributed at random, their primary effects cancel each other, while if there is a preponderance of the axes to any side, what they illustrate is a directed physical influence on the medium, of the same sort as (but not identical with) Faraday's influence of magnetisation on light. To simulate pure static elasticity by means of a gyrostat, it is necessary to arrange that a rotatory displacement produces a restoring torque directly proportional to the displacement, instead of to its velocity. The solution of the problem, mentioned earlier in the same year to the Electrical Engineers (*infra*), was explained in a communication in the *Comptes Rendus*, September 16, 1889, "On a Gyrostatic Adynamic Constitution for 'Ether.'" The constraints on the framework, itself of the original Foucault gimbal type, that are necessary to make the static angular momentum of a gyrostat react in this simple way, would require some space to describe; for though Lord Kelvin's dynamical nomenclature has brought non-mathematical explanation into effective grasp, it can never be quite easy. It may suffice to say that just the same device allows a horizontal spinning flywheel to steady the rolling of a boat, by introducing a restoring torque of direct elastic type proportional to the angular deviation from the vertical.

The result thus reached in the elastic theory of the æther had been, however, in the abstract sense anticipated. This type of medium, operating by rotational stiffness arising from intrinsic internal rotation, was, in fact, a mechanical model of a mathematical æther introduced into optics nearly fifty years before by MacCullagh, but repudiated at the time, except by Rankine, on the very ground that it behaved in a way that an elastic solid could not do. By a mathematical æther is here meant a medium specified by its potential energy function alone, without any attempt at providing a mechanical model of the detailed working of the medium that is thus completely determined mathematically in the form of that function. The gyrostatic model is also identical, in the same mathematical sense, with the electrodynamic æther of Clerk Maxwell. In this way it has come to pass that by making a model, with ordinary matter, of an elastic medium that has not the properties of ordinary matter, Lord Kelvin has vindicated to many minds, if not entirely to his own, the power and cogency of the impalpable procedure of mathematical analysis which can reach away without effort from the actual to the theoretically possible, and thus, for example, make a mental picture of an æther which is not matter for the simple reason that it is something antecedent to matter. This may be taken to be a partial view, so it is well to offer direct support of it by a quotation from the Presidential Address of January 10, 1889, to the Institution of Electrical Engineers, an address which one may venture to think was equal in fundamental physical suggestion to any of the great achievements of his early years:—"So that I do not admit that it is only playing at theory, but it is helping our minds to think of possibilities, if by a model, however rough and impracticable, we show that a structure can be produced which is an incompressible frictionless fluid when no gyrostatic arrangement is in it, and which acquires a peculiar gyrostatic elasticity or rigidity as the effect of introducing the gyrostats into these squares." Later on he proceeds:* "Thus, upon this solid, the effect of a constant couple is not to produce continued rotation, but to produce and balance a constant displacement, and that balance may last for any time, however long, if the rotational moment of momentum of the flywheel is but great enough." The last sentence implies the essential limitation of the model. This persistence of the steady balance demands continually increasing angular displacement of the axis of the gyrostat, so that it could not last for ever without some mechanism for recovery of direction; but if the movements are restricted to be vibratory or merely alternating, the model is adequate, and by taking its rotational momentum to be great enough the period of the alternation may be made as slow as may be desired, even so as to include an analogy of the cyclic field of a magnet lasting permanent for any assigned time.

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* 'Math. and Phys. Papers,' vol. 3, p. 509.

The two immense branches of profound physical investigation, optical and electrical science, thus meet and fuse together. Their approach can be conducted in two ways; it may proceed from the side of optics by the method of MacCullagh, with its self-consistency finally justified by Lord Kelvin's model, when that method is expanded so as to include the application to disturbances of the æther that are not simply vibratory; or it may proceed, as it did historically, from Maxwell's invention of a mathematical æther capable of accounting for electric phenomena, by the mathematical verification that its vibratory properties are exactly those of light and radiation.

The rotational rigidity of æther, thus illustrated, did not, however, console Lord Kelvin, for he could not see how bodies can be free to move through the medium. It is true that he puts it that he does not see why magnets attract each other and electrified bodies attract each other; but it is really the mechanism of the free mobility that he is in quest of, for the forces are determinate by the principles of dynamics, and could not be otherwise, whatever kind of mobility there might be.

In the Address just mentioned, quoting from his paper of 1847 on an elastic solid analogy, he states with reference to its last sentence (*supra*, p. xii), written as he remarks twenty-eight days after he had taken up the duties of his professorship at Glasgow:—"As to this sentence I can now say, what I said forty-two years ago—*must be reserved to a future paper*. I may add that I have been considering the subject for forty-two years—night and day for forty-two years . . . the subject has been on my mind all these years. I have been trying many days and many nights to find an explanation, but have not found it." But he adds in a footnote a reference to a paper just written, May 1890,* in which the *lacuna* is filled. There in Sec. 47, p. 465, in explaining the limitations of this æther, rotationally elastic in a way which can be illustrated but need not be specified, he adds:—"All this essentially involves the consideration of ponderable matter permeated by, or imbedded in, ether, and a *tertium quid* which we may call electricity, a fluid go-between, serving to transmit force between ponderable matter and ether, and to cause by its flow the molecular motions of ponderable matter which we call heat. I see no way of suggesting properties of matter, of electricity, or of ether, by which all this, or any more than a very slight approach to it, can be done, and I think we must feel at present that the triple alliance ether, electricity, and ponderable matter is rather a result of our want of knowledge and of capacity to imagine beyond the limited present horizon of physical science, than a reality of nature."

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* 'Math. and Phys. Papers,' vol. 3, pp. 450–465.

Looking back on this, however, one can see that the electron was not far off. These models, it can be held, now carry us further. Clearly if we could explain how one single atom is freely mobile through the æther, everything would be achieved; but the atom must be an ion and carry an electric field with it. There exists, it may be maintained, a satisfactory representation of how this electric field can travel in indissoluble attachment to a central nucleus—the whole arrangement called in 1894 an electron. But of the nature of the nucleus little more is yet ascertained, except that it is almost certainly isotropic as it was natural to assume, and can thus be representable as regards its influence at a sufficient distance by a spherical collocation of electricity; for all ordinary purposes of electrodynamics it remains, as then, a point-charge.

Mathematical analysis is the all-powerful resource that gets behind and away from all accidents of models and modes of visualisation in which our experience is necessarily set, back to the 'callow principles' as George Herbert called them. But however instructive it may be to revise our knowledge by its expression in terms of pure concepts free of all gross material implication, it seems safe to assert that it could never have been reached, in either of the ways mentioned above, without constant preoccupation, mental or tangible, with the modes of working of dynamical models and illustrations.

It has been mentioned already (p. xlii) that Lord Kelvin's difficulties in representing radiation-pressure, with such models of electrons as he clung to, prevented him from appreciation of the modern thermodynamics of natural radiation. A recent attempt to persuade him to look into this matter elicited the following characteristic reply, in autograph, of date May 8, 1907:—"There are certainly very wonderful 'push and pull' forces in the action of light on movable bodies in high vacuum (and also in not very high vacuum, as shown in Varley's communication to Royal Society 'Proceedings' of about 1871, demonstrating cathode torrent of 'negatively' electrified particles). I do not, however, think that there is any foundation for push and pull in Maxwell's ($\alpha$, $\beta$, $\gamma$) formulæ, or in the ($\alpha$, $\beta$, $\gamma$), (P, Q, R) of your leaves. There is great importance in all such experiments as those of Hull to which you refer, and those of Crookes at various times, and those of J. T. B., of which we are now hearing,* and of Dewar's which I believe he is to show this evening.† Great revelations are, I believe, coming early. Yours very truly, Kelvin.—I hope to continue this verbally when I see you in the evening."

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* J. T. Bottomley, 'Roy. Soc. Proc.,' March 18, 1907, on localised radiometer repulsions between the gold leaves of electroscopes, etc.

† Sir J. Dewar, 'Roy. Soc. Proc.,' June 27, 1907, on the lower limit to the action of the radiometer.

In 1884 Sir W. Thomson delivered the well-known course of lectures on Molecular Dynamics and the Wave Theory of Light at Johns Hopkins University, Baltimore, after attending the meeting of the British Association at Montreal. The papyrograph unrevised report issued in December, 1884, by Mr. A. S. Hathaway, may justly be said to have reawakened, or at any rate strongly intensified, interest in the ultimate form of the problem of æther and radiation, both in this country and abroad. It seems fair to say also that the interest and value of the lectures arose largely from the unpreparedness of their author. As his audience of American physicists fed him from day to day with the more recent experimental and theoretical results relating to selective absorption, which were largely new to him, they had before them the spectacle, on which Helmholtz had laid stress (*supra*, p. lv), of one of the great minds of the century struggling with fresh knowledge and trying to assimilate it into his scheme of physical explanation, calling up all his vivid store of imagery and analogy to aid. His auditors at the time, and his readers afterwards, thus must have considered the lacunæ and difficulties as their own personal problems in which they were assisting. Perhaps no exposition in physical science so vivid and tempting has ever been published; and for many years afterwards scientific activity in these subjects was strongly tinged by the Baltimore lectures, which transformed optics for the time from an affair of abstract mathematical equations into a subject of direct physical contemplation in close touch and analogy with the objective manifestations of ordinary dynamics.

In the preface to the authoritative edition of 1904, which in the twenty years' interval had grown to be a volume of some 700 pages octavo, Lord Kelvin in fact describes the object of the course of lectures as follows:—"I chose as subject the Wave Theory of Light with the intention of accentuating its failures, rather than of setting forth to junior students the admirable success with which this beautiful theory had explained all that was known of light before the time of Fresnel and Thomas Young, and had produced floods of new knowledge, splendidly enriching the whole domain of physical science. My audience was to consist of professorial fellow-students in physical science; and from the beginning I felt that our meetings were to be conferences of co-efficients* in endeavours to advance science, rather than teachings of my comrades by myself. I spoke with absolute freedom, and had never the slightest fear of undermining their perfect faith in ether and its light-giving waves; by anything I could tell them of the imperfections of our mathematics; of the insufficiency or faultiness of our views regarding the dynamical qualities of ether; and of the overwhelmingly great difficulty of finding a field of action for ether among the atoms of ponderable matter. We all felt that difficulties were to be faced and not to be evaded; were to be taken to heart *with the hope of solving them if possible*; but, at all events, with the certain assurance that there is an explanation of every difficulty though we may never succeed in finding it."

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* In the literal sense of the term.

He goes on to say that he had now, in 1904, virtually got to the bottom of the difficulties of 1884. He thinks, too, that in the wider field of æthereal phenomena *everything non-magnetic* can be explained "without going beyond the elastic-solid theory," but *nothing magnetic.* "The so-called electro-magnetic theory of light has not helped us hitherto: but the grand object is fully before us of finding comprehensive dynamics* of ether, electricity, and ponderable matter, which shall include electrostatic force, magnetostatic force, electro-magnetism, electro-chemistry, and the wave theory of light."

His purely scientific activity from 1884 onwards hinged largely on the production of the definitive edition of these lectures, which, in terms of the remarks just quoted, had raised up in front of him all the difficulties in modern optical and general æthereal theory. The resulting volume, with its numerous insertions, including most of pp. 280–468, and the twelve Appendices occupying pp. 468–700, may take rank in fact as virtually Volume IV of the 'Mathematical and Physical Papers.' Among the vast array of new and recent material collected into the volume there may be mentioned the following: theory and observation on the opacity of air and gases, reflexion from diamond and from metals, his various attempts at elastic solid vibratory theories of the æther, rotation of the plane of polarization combined with double refraction, waves on water and in dispersive media with the residual disturbance they leave behind, waves raised by wind or by ships, the total mass of the material universe, various theories of electrons or electrions as he preferred to call them; also much regarding molecular tactic of crystals and the resulting dynamics, this time on a Boscovichian foundation. The Royal Institution Lecture of 1900 on 'Nineteenth Century Clouds over the Dynamical Theory of Heat and Light' is also included; these difficulties he there reduces to two: the difficulty regarding the motion of matter through æther, which he thinks is "not wholly dissipated," and the difficulty about the frittering away of the energy of gaseous molecules among their numerous periods of free vibration, which he solves in what may possibly be held to be the natural way, by denying the proofs.

An estimate of Lord Kelvin's influence on modern geology has been contributed for this Notice by his lifelong friend, Sir Archibald Geikie :—

"Throughout his life Lord Kelvin took much interest in the progress of geology. From the year 1844 onwards for some eighteen years, he watched with increasing impatience the spread of the doctrines of the Uniformitarian School, which reached the height of its influence about the middle of last century. At length in the year 1862 he broke silence on the subject, declaring the doctrines of that school to be opposed to physical laws. Three years later he gave greater emphasis to his antagonism by publishing a short paper with the uncompromising title, 'The Doctrine of Uniformity in Geology briefly refuted.' Again in 1868 he returned to the attack and brought forward additional lines of argument in support of his charge that British popular geology stood 'in direct opposition to the principles of natural philosophy' and required 'a great reform.'

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* *Cf.*, however, pp. xi, xviii–xxiv, lxv, *supra.*

"It was one of the accepted tenets of the Uniformitarian School that the range of past time available for the explanation of the phenomena of geology was unlimited. But by arguments drawn from the origin and age of the sun's heat, the internal heat and rate of cooling of the earth, and the tidal retardation of the earth's rotation, Kelvin fixed limits to the possible age of our planet. At first he maintained that this age could not be less than twenty millions of years nor more than four hundred millions. In his latest writings on the subject he restricted the time to between twenty and forty millions.

"His papers have given rise to a prolonged controversy and no final agreement has yet been reached. But these papers have profoundly influenced the course of modern geological speculation. They roused geologists from their comfortable belief in the limitless ages of the past, and led them to revise their estimates of the value of geological time. Lord Kelvin's insistence greatly helped to tone down the extreme uniformitarian views which were in vogue half a century ago. Even those geologists, palæontologists, and biologists who most keenly dispute the validity of the arguments whereby he sought to compress the antiquity of the globe within limits that seem too narrow for the evolution of geological history, must admit that in turning the brilliant light of his genius upon this subject he did a notable service to the progress of modern geology."

Little has been said here with regard to Lord Kelvin's masterful and most effective preoccupation with the development of modern electric engineering, which has now almost completed the transition from the age of steam to the age of electric power. In this new branch of applied science, his active perception of the essentials of progress assumed the form of generalship: most of the details of progress naturally came from others, but he was ready always to emphasise the salient problems, and to acclaim, early and enthusiastically, such nascent inventions as would be pertinent to their mastery. An example is afforded by the emphasis with which he hailed the invention of the original Faure storage cell or accumulator,* which promised to supply the improvements (including the subdivision of a large storage battery to play the part of a step-down transformer, not yet practically effective) then necessary for economical development of the electric generation of power. This subject came particularly to the front in his Presidential Address in 1881 at York to the Physical Section at the Jubilee Meeting of the British Association, "On the Sources of Energy in Nature available to Man for the Production of Mechanical Effect," which almost repeats the title of his early paper of 1852, but is this time concerned with the practical utilisation of these sources, now rapidly ripening, whereas the earlier discussion related to their philosophical detection and estimation. In this Address, after referring to Siemens'

* 'Brit. Assoc. Report,' 1881, p. 526.

suggestion, three years previously, of the electrical transmission at high potential of the power of Niagara Falls, itself resting, as he remarks, on Joule's early experimental discovery that in an electromagnetic engine as much as ninety per cent. of the energy of the driving current can be utilised, he proceeds to summarise his own conclusions regarding economy of transmission over long distances, as communicated in the form of evidence to a Parliamentary Committee two years before. The brief paper, now classical in electro-technics, then communicated,* "On the Economy of Metal in Conductors of Electricity," is an early notable instance of the blending of economics with exact physics: the solution of the problem "would be found by comparing the annual interest of the money value of the copper with the money value of the energy lost annually in the heat generated in it by the electric current. The money value of a stated amount of energy had not yet begun to appear in the City price lists." He shows that the gauge to be chosen for the transmitting conductor does not depend on its length, but solely on the strength of the current to be employed. He was much concerned also in the early evolution of dynamos (the term had been introduced by him about this time as a contraction for dynamo-electric machine), the designing of which was to become entirely effective a few years later by means of the graphical methods introduced by Hopkinson. Perhaps the earliest domestic installation of electric lighting in this country was the experimental one which he established in his house at the University of Glasgow; while one of the early public installations was the one, still in operation, which he presented, in connexion with the celebration of the six hundredth anniversary of the foundation of that most ancient house, to his College in Cambridge, which had been able, under new statutes, to re-elect him to the Fellowship that he had vacated long before on his marriage.

The introduction of heavy currents and voltages in engineering required the provision of suitable instruments of measurement. This was always a congenial task: his graded series of current-weighers or ampere-meters, and of volt-meters—embodying those theoretical principles of adequate support free from constraint or strain, in mechanical design, on which he always insisted, to the great improvement of general practice in such matters—have proved to be of fundamental service wherever exact measurement is essential.

His interests ramified into all departments of human activity: even his physical writings were often relieved by play of allusion to literature and history. In his later years he took an active and zealous part in political affairs, and attended regularly the sittings of the House of Lords. In his undergraduate days he was one of the founders of the Cambridge University Musical Society, playing the French horn at its opening concerts in 1843. Later he published some observations† on the beats of imperfect harmonics of simple tones, tending to a conclusion different from that of Helmholtz which referred the beats to combination tones.

* 'Brit. Assoc. Report,' 1881, pp. 526–8.

† 'Roy. Soc. Edin. Proc.,' 1878.

All this activity implied a robust constitution. As an undergraduate at Cambridge he found time to take a keen interest in manly sports, rowing in the Peterhouse boat, which had second place on the river, and winning the Colquhoun Sculls, then, as now, one of the main objects of athletic ambition. Afterwards he was expert at curling until a serious accident on the ice stopped the pursuit, and left him slightly lame for life. His subsequent yachting and cable-laying experiences have been already referred to.

The general impression produced, at first sight, by the four volumes, containing the collected scientific papers up to 1860, might well be a somewhat vague notion of desultory, though profound, occupation with the ideas that were afterwards to be welded by more systematic expositors into our modern theoretical knowledge of mechanical and electrical and optical philosophy. At first glance, the exposition in characteristically practical terminology might even suggest that these papers were concerned with the engineering achievements by which he is most widely known, as much as with new theoretical foundations for physical science. Closer attention has compelled the conclusion that the results of his activity in the early period from 1845 to 1856 are perhaps unique in modern scientific annals; at any rate, there can have been few parallels since Newton and Huygens and their great predecessors. It is said that Lagrange qualified his profound admiration for the genius of Newton by the reflexion that only once could it be given to a mortal to have a system of the stars to unravel. Somewhat in the same way one might imagine the reflexion of a seer of the future, that it can hardly be given again to a man of genius to have, in his first dozen years of creative intellectual activity, the ideas and discoveries of a Carnot, a Faraday, and a Joule, to interpret and develop for mankind.

His only peer in general physics in those early days, as also later if we exclude his own disciples, was perhaps Helmholtz. They began their careers of investigation about the same time, but at first their paths did not lie much together. For in his early years Helmholtz's professional work was that of a physiologist, though in the essay on the Conservation of Energy he revealed, in 1847, his true bent as a leader in the exploration of the underlying principles connecting the different departments of the fundamental science, general physics. By the time this famous essay came into Thomson's hands in 1852, he had himself travelled, with Joule's assistance, as far as it reached, if we except some special applications; but much more, he had in fact already dug down, on the inspiration derived from Carnot, far into the true foundations of the doctrine of Energy as available and recognisable to man, evolving from it ideas now familiar but then of revolutionary significance, as regards both dynamical science and cosmic evolution, of which no one up to that time had any definite notion. The saving virtue of physical or any other genuine science is that the most essential discoveries of one generation become worked up so as to be obvious and almost axiomatic to the next. The charm of the study of scientific history is thus to trace the beginnings of creative ideas, to see how slight sometimes was the obstacle that delayed the discovery of a new field of knowledge; though here the temptation to read back our own refined knowledge into the past lays many snares. In no part of science is this interest greater than in the doctrine of Available Energy; the generality of outlook, leading to recasting of the fundamental ideas regarding physical force and power, which was secured by Thomson away back in the fifties, is on the least favourable view a matter for wonder.

In the years following, the powers of Helmholtz were concentrated largely on his great task of the exploration of the physical foundations of the activity of the senses, a subject of fundamental importance because they supply our only outlook into the external world; while Thomson's efforts were employed in the problem, then urgent and preparatory to Maxwell, of the dynamical interpretation of the ideas of Faraday, and in the creation of the fundamental science above referred to, which constitutes Thermodynamics in its widest sense, the all-pervading doctrine of Available Physical Energy to which it seems appropriate that Rankine's name Energetics should belong. In later days of close friendship their fields of activity had much in common, Helmholtz apparently often brooding over, and developing into fuller and more varied aspects, fertile points of view, such as the influence of wind and surface-tension on waves, and the generalisation of dynamics by the inclusion of latent cyclic motions, that had been already thrown off in more summary fashion by his colleague. On the institution of the Helmholtz memorial medal, the first award was to Lord Kelvin.

In a letter to Tait in 1876,* who was preparing a biographical notice for 'Nature,' Helmholtz had given an estimate of the work of his friend at that period. "His peculiar merit, according to my own opinion, consists in his method of treating problems of mathematical physics. He has striven with great consistency to purify the mathematical theory from hypothetical assumptions that were not a pure expression of the facts. In this way he has done very much to destroy the old unnatural separation between experimental and mathematical physics, and to reduce the latter to a precise and pure expression of the laws of phenomena. He is an eminent mathematician, but the gift to translate real facts into mathematical equations, and *vice versâ*, is by far more rare than that to find the solution of a given mathematical problem, and in this direction Sir William Thomson is most eminent and original. His electrical instruments and methods of observation, by which he has rendered, amongst other things, electrostatical phenomena as precisely measurable as magnetic or galvanic forces, give the most striking illustration how much can be gained for practical purposes by a clear insight into theoretical questions; and the series of his papers on thermodynamics, and the experimental confirmations of several most surprising conclusions deduced from Carnot's axiom, point in the same direction."

We have seen the hints and principles thrown out by Thomson in such profusion fructify in patient development by other great investigators, so that it would be difficult to name a branch of modern physical science in which his activity has not been fundamental. In one phase of his thought, it becomes cosmical and transcends experimental aids. All through life his ideas were wont to range over the immensities of the material universe, reaching back to its origin and onward to its ultimate fate. In his youth he established the cardinal principle of inanimate cosmic evolution, as effected through the degradation of energy, which determines the fate of worlds, and is the complement of the principle of evolution in organic life which came to light at about the same time. In another aspect of this principle, asserting that the trend of available energy must always be downwards, it has developed into the key to the course and the equilibrium of voltaic and chemical change, and to all other branches of physical knowledge in which the atomic nature of matter is the pervading influence. The greatness of the revolution thus effected in physical science, and in its industrial applications which are in strict relation to this available energy, requires no emphasis. The magnitude of the advance brought by the mere enunciation of the principle of dissipation is to be measured by the very inevitableness of this law to our present modes of thought; it is difficult now to recognise the limitations that must have belonged to the time when its formulation caused such surprise and wonder.

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* 'Nature,' vol. 14, 1876, p. 388.

At the end of his strenuous career his thoughts reverted again to these problems of the origin and destiny of material things. Novel considerations were brought to bear, with intellectual vigour appropriate to youth, to demonstrate even the finiteness of the material universe—such, for example, as the darkness of the firmament and the moderate magnitude of the relative velocities of the most distant stars. In the last weeks he pondered over the remote history of our own planet, and reasoned with striking force and lucidity, as may be read in a posthumous paper, on the antiquity of its continents and oceans, reaching back possibly to the time when the Moon separated from the Earth.

In this Notice the chief aim has been to set out a connected historical view of the course of Lord Kelvin's scientific activity and its relation to his contemporaries. No attempt has been made to describe the charm of his personality. That has been recognised long ago by the whole world; for many a year the ordinary restrictions of nationality have had little application to him; he has been venerated and acclaimed wherever scientific investigation is appreciated. No instance in his long career can be recalled in which he asserted for himself any claim of priority in intellectual achievement; rather his attitude has always been to show how much he had learned from his colleagues, and how much he expected to derive from them in the future. In this regard there is just time to interpolate an extract from the fine appreciation by Lord Rosebery, his successor in the chancellorship of the University of Glasgow, delivered in his installation address*:—"In my personal intercourse with Lord Kelvin, what most struck me was his tenacity, his laboriousness, his indefatigable humility. In him was visible none of the superciliousness or scorn which sometimes embarrass the strongest intellects. Without condescension, he placed himself at once on a level with his companion. That has seemed to me a characteristic of such great men of science as I have chanced to meet. They are always face to face with the transcendent mysteries of nature. . . . Such labours produce a sublime calm, and it was that which seemed always to pervade Lord Kelvin. Surely, in an age fertile in distinction, but not lavish of greatness, he was truly great. Individualism is out of fashion. . . . But great individualities such as Lord Kelvin's are independent of the pressure of circumstance and the wayward course of civilisation."

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* 'The Times,' June 13, 1908.

It is unnecessary to attempt any list of the distinctions and awards which came to him in the course of years; it suffices to say that there was probably no honour open to a man of science that was not at his disposal. Abundant personal record is and will be available in appreciations by his colleagues, who were all his friends; for example in the masterly estimate by G. F. FitzGerald contained in the memorial volume reporting the proceedings in celebration of the Jubilee of his Professorship at Glasgow in 1899. In deference to the strikingly unanimous desire of his countrymen of all classes, and amid touching tributes from his colleagues in other nations, he was laid finally to rest in historic ground, on December 23, 1907, alongside his great exemplar Sir Isaac Newton, in Westminster Abbey.

J. L.

# OBITUARY NOTICES

# OF

# FELLOWS DECEASED.

(*Continued.*)

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# P. J. C. JANSSEN, 1824—1907.

PIERRE JULES CÉSAR JANSSEN was born in Paris on February 22, 1824. Son of a musician, grandson of an architect, both distinguished, an artistic career was arranged for him, and he began the study of painting with great diligence, but he was so forcibly attracted by physical science that he eventually took up his studies in that direction, always, however, retaining a great taste for art. He was, to a large extent, self-taught, as his father lost his property; Janssen for some years supported himself by working in a bank and in giving private lessons. Every spare hour, however, was spent at the Sorbonne in following the lectures of Cauchy, Chasles, Le Verrier and others, and on Sundays he studied at the Conservatoire. In 1852 he obtained the Degree of Licentiate of Mathematical Science, he was répétiteur at the Lycée Louis le Grand in this year, and, in 1855, won the diploma of Licentiate of Physical Science, after which he eventually took his degree at the University.

In 1857 the Minister of Public Instruction, in spite of Janssen's lameness, which he owed to the carelessness of a nurse, put him in charge of an expedition to determine the course of the magnetic equator across Peru. He was accompanied by the two brothers Grandidier, his pupils. Alfred Grandidier became, subsequently, an explorer of Madagascar, and was made a Member of the Academy. Janssen unfortunately contracted fever in traversing the swamps and forests, and was obliged to abandon his measurements. For many months he was so seriously ill that the return to Europe could not be undertaken till the next year. On his return he took another teaching position at Creusot, and, while so engaged, he prepared his thesis for the Degree of Doctor of Physical Science. This dealt with a remarkable study of the eye, in which he demonstrated that the media of the eye possess the property of absorbing dark radiant heat, and only allow those rays to reach the retina which are necessary for vision. He obtained the degree on the strength of this in 1860; and it was Kirchhoff and Bunsen's discoveries at that time which deflected him from Ophthalmology to Spectroscopy, to which, in the main, the rest of his life was devoted. His spectroscopic studies began by enquiries into atmospheric absorption.

First a large dispersion was employed to study the solar spectrum at varying altitudes. He had already built a private observatory in 1862, on a belvedere on the top of his house in the Rue Labat (Montmartre). From 1862 to 1864 he was occupied with missions to Italy and the Alps in connexion with this work, to get the least density of atmosphere by observing at a high altitude. Eight days on the Faulhorn, in 1864, were enough to enable him to state that the telluric rays were much less visible at that elevation.

In the same year experiments were undertaken near Geneva for the same purpose. The spectrum of a large bonfire at Nyon was examined by a spectroscope there and afterwards located in the tower of St. Peter's Church at Geneva, twenty-one kilometres away over the lake. The telluric lines were observed and that they were due to absorption by the atmosphere was strongly suggested; later work at La Villette ran them home to water vapour.

From 1865 to 1871 Janssen held the post of Professor of General Physics at the Special School of Architecture, but, as we shall see, he found time to continue his spectroscopic work and expeditions.

In 1867 he went to Trani, in Italy, to observe the eclipse of the sun, with special reference to the thickness of the reversing layer in the sun's atmosphere. He was in the Greek Archipelago with Fouqué at the time of the eruption of Santorin, where he observed the sodium and hydrogen lines in the flames from the lava. He ascended Mount Etna, where he detected the probable presence of water vapour in the atmospheres of Mars and Saturn. Also in 1867 he went to the Azores to make optical researches with Charles Sainte-Claire Deville; while crossing Spain and Portugal he spent some time in securing magnetical observations. In 1868 he observed the eclipse of the sun at Guntoor, in Hindustan, on behalf of the French Government, the Académie des Sciences, and the Bureau des Longitudes.

This was the occasion on which he discovered that by using a spectroscope the sun's surroundings and their chemistry could be observed without the intervention of an eclipse. During the eclipse he was struck with the great brightness of the lines, chiefly of hydrogen, which were visible, and it struck him that they ought to be visible at other times. The weather clouded after the last contact, so nothing could be done that day; but he rose at 3 A.M. the next morning, and, having arranged his apparatus in the way he had thought out, with a radial slip adjusted for C, he soon saw a bright line in prolongation of the dark C in the spectrum of the photosphere.

Referring to these observations in a letter to his mother on September 6, he wrote, "Je lis dans un livre fermé jusqu'ici pour tous . . . "

The French Government, recognising the importance of the discovery of the new method of sun observation, at once sent out to Janssen the Cross of the Legion of Honour. The Academy of Sciences awarded him the Lalande prize, quintupled in value, and further, at the suggestion of M. Dumas, struck a medal to commemorate the event.

A year afterwards, in 1869, Janssen suggested at the Meeting of the British Association at Exeter the use of two slits on a rotating spectroscope so that a complete image of the prominences could be photographed on a fixed plate. Here we have the germ of the idea which has since been utilised by both Hale and Deslandres with such success.

In 1870 there was an eclipse visible in Algeria, and in 1869 Janssen commenced his preparations to observe it. Unfortunately, before they were completed Paris was besieged by the German Army. Under these circumstances the British Eclipse Committee, who had invited Janssen to join their expedition, begged the intervention of the Foreign Office. Lord Tenterden took the greatest interest in the affair, and ultimately Prince Bismarck granted a passport for Janssen to pass through the German lines with his instruments. It is not quite clear whether this ever reached Janssen, but in any case he did not take advantage of it.* He left Paris in a balloon, the "Volta," with a single mariner as companion, on December 2. It was his first ascent, and was not without its difficulties; the proper "balancement du ballon" was not easy to secure. All, however, turned out well so far as getting to his appointed station went; but for him, as for most of the English and American observers, work was prevented by clouds.

In spite of his lameness, a good part of the next year was spent in long voyages: in 1871 to India to observe another eclipse, in 1874 to Japan for the Transit of Venus, and in 1875 to Siam, to observe still another eclipse. While at home he continued his spectroscopic observations, and, in relation in particular to the Transit of Venus, he developed the application of photography with the idea, as he expressed it, "la plaque photographique sera bientôt la véritable rétine du savant." The "revolver photographique," then designed by him to automatically obtain a rapid succession of instantaneous pictures of Venus transiting the sun's limb, is the origin of the now well-known cinematograph.

The progress of solar studies carried on by Janssen and others had been so immense that the French Government decided to establish an observatory for solar physics. M. Duruy, the Minister of Public Instruction, first took up the question in 1869, and the Pavillon de Breteuil was granted by the Emperor for the purpose; but then came the war of 1870, and it was not till July 22, 1874, that the proposal was laid before the National Assembly to create near Paris a special observatory for astrophysical inquiries. The Minister submitted the proposal to the Academy of Sciences (August, 1874), which body not only expressed their entire adhesion to the project but strongly urged its prompt realisation. There was a provisional installation in Montmartre (Boulevard Ornano) from 1876 to 1879, until the question of site was settled. The choice lay between the two State domains of Malmaison and Meudon. The latter, still occupied by the troops, was ultimately determined upon. As the château had been burnt after the war, Janssen installed the instruments in the dependencies (October, 1876), where, indeed, he himself lived till his death. Two years afterwards the lower part of the château was restored, and a large dome of 18·50 m. diameter erected on it, which now contains one of the largest refractors in Europe. The other instruments were erected in observatories distributed in the grounds. The divisions of the stalls in the stables, a range of buildings 100 metres long, were utilised as supports for lengthy tubes containing the gases, the study of the absorption of which formed part of the physical programme.

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* On this point Madame Janssen has been good enough, in reply to an inquiry, to send me the following information:—

"Monsieur Janssen a eu connaissance des démarches faites par votre Gouvernement pour obtenir de M. de Bismark son libre passage, mais il ne pouvait moralement en profiter; ayant reçu du Général Trochu et de Jules Simon la mission verbale d'aller trouver Gambetta à Tours où siégeait le Gouvernement de la défense nationale, il s'empressa de partir sans attendre la réponse de M. de Bismark. Mais il n'en est pas moins resté, croyez le bien, infiniment reconnaissant au Gouvernement Anglais dont il a toujours éprouvé l'estime et la générosité, et à vous-même qui aviez pris grande part dans cette affaire.

"Aussitôt, arrivé à Tours, il est allé voir Gambetta avant de se rendre en Algérie, et pendant bien des années, il ne lui était pas possible de laisser connaître les circonstances particulières de son départ."

The first work done at Meudon was a special photographic study of the sun's surface. The instrument employed was an object-glass of 5 inches aperture, with a secondary magnifier giving images from 12 to 18 inches diameter, using the light near the solar line G and exposures from 1/1000 to 1/3000 of a second. The photographs thus obtained were a revelation, and enabled Janssen to largely increase our knowledge of the currents in the sun's atmosphere.

On the experimental side, Janssen returned to his inquiries as to the origin of the telluric bands, this time with special reference to oxygen, and the long tubes to which attention has been called were utilised in the research. The carrying out of these and allied branches of work took some five years after the installation of the observatory at Meudon. It was not till 1882 that we find him taking part in another series of long voyages: 1882, to observe the transit of Venus at Oran; 1883, to observe still another solar eclipse on Caroline Island in the Pacific; 1884, to plead for a neutral meridian at the International Conference at Washington. On his way back from Caroline Island he repeated his spectroscopic observations at Santorin by touching at the Sandwich Islands to investigate Mauna Loa. In the emanations from the lava lake he detected spectroscopically sodium, hydrogen, and hydrocarbon vapours.

His voyages for some time onwards from 1888 had for one main object the continuance of his investigations into the bands of oxygen in the red end of the solar spectrum experimentally studied at different pressures at Meudon. In the early observations at Geneva and Nyon it was a question of water vapour, and that point was settled; but with regard to the oxygen bands it was necessary to deal with as little of the earth's atmosphere as possible, and therefore to make observations of the sun's spectrum at a great altitude. In spite of his lameness we find him doing this work at the Grands Mulets in 1888. Here he found the bands less intense; and Janssen's way was to endeavour to carry on the work in an observatory higher still, and for this nothing less than the highest point in Europe, that is, the summit of Mont Blanc itself, nearly 1800 metres higher than the Grands Mulets, would satisfy him. Thanks to the munificence of M. Bischoffsheim, the founder of the Nice Observatory, Prince Roland Bonaparte, M. Eiffel, and of Janssen himself, a Mont Blanc Observatory was determined upon in 1890, and was erected by 1893, in which year, and again in 1895, Janssen was carried up the mountain in a special form of *chaise à porteurs*. The labours of himself and others were rewarded by the result of the observations.

The oxygen bands were at their feeblest at the greatest height, and it was, therefore, the atmosphere of the earth and not of the sun which gave rise to them.

His last eclipse expedition was to Spain in 1905; his last journey was to observe the eruption of Vesuvius in 1906, when he was eighty-two years of age. His last visit to England was to take part at a Meeting of the Solar Union at Oxford, in the autumn of 1905, at which he was elected President d'honneur. His last appearance was at another Meeting of the Solar Union, held at his invitation at Meudon, in May, 1907.

The preceding account of Janssen's various activities will have shown that in his death the world of Science has lost a man of the first order. Imagination, persistence, unflagging energy, and height of aim were always present; but to those who knew him best, both in his laboratory and in the world among his confrères, his beautiful nature overshadowed his scientific reputation. He was never so happy as when descanting on the successes of his brother pioneers in the new field of work which he had been among the first to till. For him there was no question of personal achievement, the advance of knowledge, by whomsoever made, was his chief desire and delight. With such qualities of mind and heart, his declining years were made very easy for him; the devoted affection of his wife, who accompanied him in many of his voyages, and daughter, shielded him from all cares and anxieties in his modest home in one of the dependencies of the château, while the visits of affectionate friends kept his life bright. The French Government suspended their retirement rule in his favour, so that he was enabled to breathe his last in the observatory he had founded and made famous.

Janssen died full of honours. He was Commander of the Legion of Honour; he was elected a Membre de l'Institut in 1873; he was the oldest Member of the Academy of Sciences, having succeeded Laugier. He was also a Member of the Bureau des Longitudes, and had received the Lalande Medal. The Academies of Rome, St. Petersburgh, Brussels, and Washington, and the learned societies of many countries enrolled his name on their list of Fellows. In this country he was a Foreign Member of the Royal Society (1875), from which he received the Rumford Medal for his researches in 1877; Edinburgh made him an LL.D. of that University, and in 1872 he was elected an Associate of the Royal Astronomical Society. He also received the Gold Medal of the Royal Geographical Society for his many voyages. This he afterwards had melted down, and from the proceeds founded an annual prize which bears his name at the Société de Géographie.

He died December 23, 1907, and was buried in Père-la-Chaise.

Wolf, Radan, Deslandres, de Lapparent, Pector, de Fonvielle, le Commandant Paul Renard, and Dr. Faveau de Courmalles, representing various scientific organisations, each pronounced a discourse over the grave.

N. L.

## THOMAS ANDREWS, 1847—1907.

The late Mr. Thomas Andrews was born in 1847, and educated at Broombank School, Sheffield. He studied chemistry under the late Dr. Allen, and early showed a deep interest in scientific research. He was also trained in practical metallurgy and engineering under his father, upon whose death, in 1871, he became proprietor of the Wortley Ironworks.

Mr. Andrews took up the investigation of scientific metallurgy at a time when workers in this field were comparatively few, and up to the time of his death devoted a large portion of his time to scientific research. The results of these researches are embodied in forty papers, published in the Proceedings of the Royal Societies of London and Edinburgh, the Minutes of the Institute of Civil Engineers, the Proceedings of the Society of Engineers, and other Scientific and Engineering publications. As a result of these investigations, Mr. Andrews acquired the reputation of an expert upon metallurgical questions; he was consulted by His Majesty the late King of the Netherlands, the Board of Trade, the Admiralty, and many leading railway and naval companies, upon matters relating to iron and steel, and in the course of this work examined and reported upon many serious accidents caused through the breakage of steel.

Mr. Andrews was one of the first to take up the microscopic examination of metals, and contributed many papers upon this subject, paying particular attention to the crystalline structure of iron and steel, and the manner in which sulphide of manganese was distributed in steel forgings.

Previously he had exhaustively studied the corrosion of metals, and the action of tidal streams upon iron and steel. He advanced proof to show that metals which were strained or distorted by cold work were less liable to be acted upon by sea water than in their ordinary or soft condition.

Another long and costly series of experiments was made upon the effect of temperature on the strength of axles. In these experiments over 300 tons of snow were used in the making of freezing mixtures, and no less than 286 railway axles and forgings, weighing more than 41 tons, were tested and destroyed in the various experiments.

The following were Mr. Andrews' general tentative conclusions in the foregoing investigation:—

1. The impact tests with an "energy" of 10 foot-tons on axles at a temperature of 212° F., compared with results at 7° F., indicated an increase of endurance at the higher temperature of about 235 per cent.

2. The impact tests with an "energy" of 15 foot-tons on axles at a temperature of 120° F., compared with results at 7° F., showed an increase of endurance at the higher temperature of nearly 120 per cent.

3. The impact tests with an "energy" of 10 foot-tons on axles examined at a temperature of 100° F., when contrasted with results obtained at 7° F.,

demonstrated an increase of resistance at the higher temperature of about 43 per cent., and this increase was, within certain limits, in proportion to the increase of temperature.

4. The impact tests with an "energy" of 5 foot-tons on axles at a temperature of 100° F. gave an increase of resistance of about 138 per cent., compared with the results on axles similarly tested, but at a temperature of 0° F.

5. The impact experiments with an "energy" of $2\frac{1}{2}$ foot-tons applied to axles at a temperature of 100° F., compared with experiments at 0° F., showed an increased resisting power to concussion at the higher temperature of nearly 88 per cent.

Another series of delicate physical experiments dealt with the electro-chemical and magnetic properties of iron and steel, and the so-called "passive state" of these metals.

A few years ago Mr. Andrews gave a course of lectures to the engineering students of the University of Cambridge upon the microscopic examination of metals, and its relation to engineering, which led to several important researches being undertaken in the University laboratories. Mr. Andrews took a deep interest in Technical Education and was a Governor of the Sheffield Technical School for many years. He also was keenly interested in the foundation of the New University of Sheffield, and, as a member of the Council, took an active part in the development of the University.

Mr. Andrews had been in failing health for the past two years, but up to the time of his death on June 19, 1907, was able to take an active interest in his metallurgical and educational work.

In recognition of the value of his work he was awarded the Telford Medal and three Telford Premiums by the Institution of Civil Engineers, the Bessemer Gold Medal, Bessemer Premium, and the Society's Premium of the Society of Engineers, and the Medal of the Franklin Institute.

Amongst his many papers the following, perhaps, may be cited as the most important, viz.:—

"Galvanic Action between Wrought Iron, Cast Metals, and various Steels during long Exposure in Sea Water," 'Min. Proc. Inst. C. E.,' vol. 77, 1883–4.

"Corrosion of Metals during long Exposure in Sea Water," 'Min. Proc. Inst. C. E.,' vol. 82, 1884–5.

"The Relative Electro-chemical Positions of Wrought Iron, Steels, Cast Metals, etc., in Sea Water and other Solutions," Roy. Soc. Edinburgh.

"Effect of Temperature on the Strength of Railway Axles," Parts I, II, III, 'Min. Proc. Inst. C. E.,' vols. 87, 94, 105.

"Effect of Chilling on the Impact Resistance of Metals," 'Min. Proc. Inst. C. E.,' vol. 103, 1890–1.

"Microscopic Internal Flaws in Steel Rails and Propeller Shafts," 'Engineering,' January 17, 1896.

"Microscopic Effects of Stress on Platinum," 'Proc. Roy. Soc.,' vol. 70, 1902.

"Effect of Segregation on the Strength of Steel Rails," Soc. Engineers, 1902.

"Microscopic Structure of Gold and Gold Alloys," 'Engineering,' 1898.

"Electro-Chemical Effects on Magnetising Iron," 'Proc. Roy. Soc.,' vols. 42, 44, 46, and 52.

"The Micro-Metallography of Iron," 'Proc. Roy. Soc.,' vol. 58.

J. E. S.

## LIEUTENANT-GENERAL SIR RICHARD STRACHEY, G.C.S.I.,*

### 1817—1908.

THE Strachey family was originally settled at Saffron Walden, but in the seventeenth century they moved to Sutton Court, Somerset, their present seat. For generations many members of the family have been remarkable for their varied ability, but the name is best known from the Stracheys who have filled important civil and military posts in India. For nearly a hundred and fifty years there has never been a time without a Strachey in some important position there. Amongst these may be mentioned Sir Henry Strachey, the first baronet of the family, who was Secretary to Lord Clive, and Sir John Strachey, the celebrated Indian Finance Minister, who died only a few months before his elder brother, Sir Richard.

Richard Strachey was a grandson of the first baronet, being the third son of his second son, Edward, of the Bengal Civil Service. He was born at Sutton Court on July 24, 1817. In 1834 he entered the Indian Military Academy at Addiscombe, and two years later he left it at the head of his class, with a commission in the Bombay Engineers. On reaching India he served for a short time at Poona, and in 1839 he was transferred to the Bengal Engineers. In the following year he was posted to the Jumna Canal, one of the first large irrigation works undertaken by the Indian Government. He was next transferred to Hurdwar as executive engineer of the Ganges Canal, which had been begun in 1842 by Sir Proby Cautley. This peaceful work was interrupted by the first Sikh War (1845–6), and Strachey was posted to Sir Harry Smith's division. At the Battle of Aliwal (January 28, 1846) his horse was shot under him. He made the authoritative plan of the Sikh position before the decisive Battle of Sobraon (February 10), and assisted in the construction of the bridge across the Sutlej, whereby the British troops entered the Sikh territory. He received his brevet majority for these services, and, when peace was made, returned to his work on the Ganges Canal. But frequent attacks of fever compelled him to go to the hill station of Naini Tal, a retirement which led to some interesting scientific work, to which we shall refer later.

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* In this article free use has been made of the obituary article in the 'Times,' even where the exact words are not quoted. The author has also made use of articles in 'Nature,' in the 'Geographical Journal,' and of letters in the 'Spectator'; he has further relied on his own private knowledge and correspondence.

After a short period of irrigation work in Bundelkhund, Strachey became in 1856 Acting Secretary to Government in the Public Works Department. In the great convulsion of the Indian Mutiny in the following year all peaceful work was naturally in abeyance, and when Mr. (afterwards Sir) John Peter Grant was placed in charge of the provisional government of the Central Provinces, with headquarters at Allahabad, he chose Strachey as his Secretary in all departments of Government.

Subsequently Lord Canning himself came to Allahabad, and Grant went to Calcutta to take up another post. Strachey, however, still remained at Allahabad, and was occupied in the restoration of that city, which had suffered much in the Mutiny. His ability was by now universally recognised in India, and he acted for a time in Lord Canning's Government as Secretary of Public Works, on behalf of Sir Henry Yule, although his actual official title was Consulting Engineer for Railways.

" In 1866 he became the first Inspector-General of Irrigation, a post he " vacated three years later to resume his old post of Secretary for Public " Works. Altogether for more than twenty years he exercised a commanding " influence on the policy and administration of this important department, " and a very strong one on the general administration. . . . 'It is to him,' " wrote his brother Sir John, a few years ago, 'that India owes the initiation " of that great policy of the systematic extension of railways and canals, which " has been crowned with such extraordinary success, which has increased to " an incalculable extent the wealth of the country, and has profoundly " altered its condition.' . . . The story is told that, when he took the " first of the famous despatches, defining his policy and pressing it upon the " Secretary of State, to Lord (then Sir John) Lawrence, the Viceroy read it " through carefully without a word, put his initials at the foot, and remarked " with a shake of the head, 'Strachey, they will think me very clever.' "*

In laying out the system of Indian railways, there was a hot controversy as to the gauge to be adopted, and Strachey advocated the adoption of a narrow-gauge for thinly inhabited tracts of country. This plan was followed, although he foresaw that in many cases the narrow might ultimately need conversion to the broad gauge. In a memorandum written for the Duke of Argyll after his return from India he pointed out the superiority of the narrow gauge for military purposes. A distinguished colleague of his on the Indian Council is still of opinion that, on this question of the gauges, Strachey was wrong.

On such questions he was accustomed to argue eagerly, and perhaps in too masterful a spirit; but he was incapable of any resentful feeling when opposed, and his generous nature led him to be fair-minded in all his dealings.

On Strachey's leaving India in 1871, Lord Mayo's Government took the unusual course of thanking him for his valuable services, and on his return to England he was appointed Inspector of Railway Stores at the India Office. In 1875 he was nominated by Lord Salisbury to the Indian Council, and in 1876 he retired from the army with the rank of Lieutenant-General.

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* The 'Times,' February 13, 1908.

Although he had retired from permanent office in India he returned there in 1877 on a mission connected with the purchase of the East Indian Railway by the Government and remained until 1879, having been appointed President of the Famine Commission, and subsequently temporary Finance Minister in place of his brother Sir John.

In connexion with this stage of Strachey's Indian career, it may be well to quote portion of the eloquent appreciation by Sir Charles Elliott contained in a letter addressed to the 'Spectator,'* for this letter shows how his personality and his work were esteemed by one of his most distinguished colleagues. The letter is as follows:—

"I venture to offer what must be an inadequate tribute to the great "achievements of Sir Richard Strachey. It cannot be an adequate tribute, "for one of the most remarkable things about him was the varied and many-"sided character of his gifts. Beginning as a brilliant soldier in the Sikh "War, he rose to the summit of his profession as an engineer in the "construction of irrigation canals and railways, and in defining the policy "which should govern the programme of their extension. He held a leading "position as a man of science in respect of botany, meteorology, geology, and "geography; and towards the close of his Indian career he showed qualities "of the highest statesmanship in dealing with the questions of famine "policy, and with the problems of finance and exchange, when the fall in the "price of silver threatened to plunge India into an abyss which would "engulf the whole of the growth of her revenues. I doubt if anyone exists "who is competent to deal with all the various and complex facets of such "a mind, and I at least can only speak on what came under my own "observation during the time when I was fortunate enough to be brought "into close connexion with him.

"When the first Indian Famine Commission was appointed in 1878, with "Sir Richard Strachey as President, I became its Secretary, and during two "years, one of which was spent in making inquiries in India, the other in "drawing up the Report in England, I was in intimate communication with "him. He had long been deeply interested in the subject, and in the famine "of 1868—69 had drawn up for his brother a paper which Sir John, who "was then Magistrate and Collector of Moradabad, fully adopted, and which "embodied the rudiments of the main lines of policy which the Commission's "Report more fully developed. The system which it laid down as to the proper "measures for famine relief has been tested by the two severe famines of "1897–98 and 1899–1900, and has in the main held its ground. The "recommendations as to administrative changes, the creation of the "Agricultural Department, the great activity of the Forest Department

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* February 22, 1908.

" (a Department in the creation of which he had taken a leading part), the " prosecution of public works for the protection of the country against " drought, and the encouragement of diversity of occupations have been or " are still being carried out, and have been productive of immense good to the " country."

In 1889 Strachey resigned his life-tenure of membership of the Indian Council, to which he had been appointed by Lord Cranbrook, in order to take up the Chairmanship of the East Indian Railway. Under his leadership the mileage of the East Indian Railway rose from 1600 to 2700 miles, and it became one of the most prosperous railways in the world. The salary attached to the office was insignificant compared with that received by the chairmen of other great railways. It should be mentioned that Strachey was also Chairman of the Assam-Bengal Railway, and that he only resigned these positions in 1907, when nearly ninety years of age.

Sir Arthur Godley, Permanent Under Secretary of State, who enjoys an exceptional position for reviewing the careers of the men who have played important parts in Indian administration, writes thus of Strachey's services at the India Office* :—

" It is difficult to convey an idea of my opinion of them without seeming to " exaggerate. He was not only invaluable in his own special lines of Public " Works and Finance, but his knowledge and his interests were universal; there " was no department of our work on which I was not always glad to get his " opinion, especially if the subject was one of any difficulty or anxiety. His " extraordinary ability and insight, his immense knowledge of everything " relating to India, his wonderful industry and power of work, his absolute " 'straightness' and fearlessness in giving his opinion—these things give him, " in my official memory, a place apart; I have known many first-rate public " servants, but I never knew one quite like him in all these respects."

The great career which has been thus sketched would seem to be more than sufficient to fill the life of one man, and yet no mention has been made of that part of his activity which leads to the notice of his life in these pages. It seemed best to separate the accounts of the official work from that of his work in science, although the two are really closely intertwined. In the letter of Sir Charles Elliott, portion of which has been already quoted, we read :—

" It is an interesting subject for reflection to consider how much India " owes to the constitutional delicacy of some of her greatest men. . . . No " one would connect Sir Richard's keen and fiery temperament with " disease; but it was to a time when he was incapacitated from his canal " work by malarial fever that he owed his opportunity for burgeoning out " into the studies of botany and geography in the Himalayas, which led to " his honours as President of the Geographical Society and as Fellow of the " Royal Society."

It was in 1846, when, as already mentioned, he was compelled by frequent attacks of fever to relinquish his work on the Ganges Canal, that he undertook extensive explorations in the Himalayas and into Tibet. An anonymous correspondent in the 'Spectator'* writes:—

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* From a private letter.

"Starting from the plain of Rohilkand, at an elevation of about a "thousand feet above sea level, a north-easterly route was taken across the "snowy ranges, and terminating on the Tibetan plateau at an altitude of "between 14,000 and 15,000 feet on the upper course of the River "Sutlej. . . . The herbarium, which contained over two thousand species "(including cryptogams), was distributed in 1852–53 to the Hookerian "Herbarium (now at Kew), the British Museum, the Linnean Society, and "to some of the Continental museums. All the specimens were carefully "ticketed with notes of the localities and elevation at which they were "found. A provisionally-named catalogue, prepared by Sir Richard Strachey, "was printed, and a copy was sent with each distributed set of plants. "This catalogue was afterwards printed, and appeared in 1882 in Atkinson's "'Gazetteer.' . . . . At the request of Sir Richard Strachey another revised "edition was prepared by Mr. J. F. Duthie. . . . . Of the large number of new "species and varieties discovered [by him] no less than thirty-two bear his "name."

I learn from a letter from Sir William Thiselton-Dyer that this journey was made in conjunction with Mr. J. E. Winterbottom, who went to India in 1848 to make botanical collections, and that the Government of India recommended him to the charge of Strachey, who was then surveying in the Himalayas. The geographical and other results of the expedition were to have been worked out by Mr. Winterbottom, but he died in 1854, and his journals were lost. Strachey therefore published his own account of the journey in the 'Journal of the Royal Geographical Society' in 1900, and the botanical results were only published in 1906, after a lapse of nearly sixty years. Thiselton-Dyer considers this to be one of the most important documents in existence on the Himalayan flora.

The geological observations made by Strachey during this journey, and published by the Geological Society in 1851, afforded the first contribution to our knowledge of the geology of the Himalayas. He studied the glaciers of Kumaon, and established the existence of a great series of palæozoic beds along the line of its passes leading into Tibet, with jurassic and tertiary deposits overlying them. Sir Thomas Holdich writes:† "It is not too much "to say that the information acquired by Strachey in that first excursion "across the Himalayas—information geological, botanical, relating to glaciers "and snowfall . . . has never been exceeded by any one traveller." In consequence of the knowledge acquired in his travels, he was invited to write the articles "Asia" and "Himalaya" in the 'Encyclopædia Britannica.'

Previously to this journey he can have had but little knowledge either of botany or of geology, and it affords a striking testimony to his ability and energy that he should have been able to obtain such important results. As a consequence of these contributions to science he was elected to the Royal Society in 1854.

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* In a letter, signed "B.," February 22, 1908.

† The 'Geographical Journal,' March, 1908, p. 343.

On his return to England he naturally became a leading power at the Royal Geographical Society, and from 1887 to 1889 he filled the office of President. During his tenure of the Presidency he endeavoured to promote the teaching of geographical science, and he came to Cambridge to deliver a short and admirable series of lectures on the scope of geography, which were afterwards published in the form of a book. In June, 1892, the University recognised this service and his many contributions to science by conferring on him the degree of LL.D., on the recommendation of the Duke of Devonshire, Chancellor of the University, and formerly Secretary of State for India.

He also exercised great influence at the Royal Society, and served on the Council no less than four times—viz., in 1872–4, 1880–1, 1884–6, and 1890–1; and in 1880–1 and again in 1885–6 he was nominated as one of the Vice-Presidents.

He was one of the British delegates at the International Congress at Washington in 1884 for determining the Prime Meridian, and acted as one of the three Secretaries.

Great as are the services to science which have been already enumerated, we have not yet come to that branch of science to which Strachey made his most important contributions. On his return from India in 1873 he was appointed a member of the Meteorological Committee of the Royal Society, which controlled the Meteorological Office established in 1867. He was a member of Sir William Stirling Maxwell's Committee which revised the constitution of the governing body of the Office, and was a member of the Council which replaced the Committee in 1876. On his final return from India he resumed his place on the Council; on the death of Professor H. J. S. Smith in 1883 he was appointed Chairman, and he held that position until the termination of the existence of the Council in 1905 and the reorganisation of the Office on its present footing.

An article by Dr. W. N. Shaw, the present Director of the Meteorological Office, gives an admirable account of Strachey's position in meteorological science. Dr. Shaw writes as follows:—*

" My personal recollection of Sir Richard Strachey goes back to 1880, " when I was engaged upon some work for the Meteorological Council. He " was keenly interested in questions about the distribution of vapour pressure " in the atmosphere. The vertical distribution was the subject of a paper in " the 'Proceedings' of the Royal Society in 1862. My recollection is that he " had a good deal to do with disposing of an idea that I have seen attributed " to Herschel, that in reckoning the pressure of the atmosphere, water " vapour did not count . . . . .

" The distribution of vapour pressure in the atmosphere as determined by

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* 'Nature,' February 27, 1908.

" his own observations up to 18,000 feet in the Himalaya was again discussed " (in the Cambridge Lectures) . . . . . He returned to the subject of " aqueous vapour in the atmosphere again in the determination of the heights " of clouds by photographic observations at Kew ; a preliminary report on the " measurements was contributed to the 'Proceedings' of the Royal Society in " 1891, and there still exists a great store of unworked material.

" From 1897 onwards I was closely associated with Strachey in the " management of the Meteorological Office, and I speak without hesitation for " his colleagues, Galton, Wharton, Buchan, Darwin, Field, and Scott, in saying " that association with him was not the least among the privileges which " attached to membership of the Council. His clear insight into the questions " at issue, his perfect lucidity in thought and expression, the logical " marshalling of facts in the official documents which he wrote as Chairman, " will always be memorable. He had not much patience with people who " were imperfectly acquainted with the facts of a case under discussion, and " he never cared to argue with them, but difference of opinion on lines " of policy, even when ill expressed, never ruffled his serenity in the conduct " of business. From time to time while he was Chairman the office was " subject to criticism which was not always fair, but he never complained. " He was always content to attribute the criticism to want of knowledge of " the facts. He would not even let us indulge in the semi-official pastime of " abusing the Treasury. Their responsibility had to take account of an aspect " of the matter with which we were not cognisant, namely, where the money " was to come from, and we must be content to accept a judgment that had " to reckon with public opinion in its executive form as well as with scientific " aspirations. Speaking for myself as one accustomed for many years to the " details of business of College meetings and University syndicates, Strachey's " methods of transacting corporate business were a revelation.

" As regards his later contributions to the science of meteorology, some " words of explanation are necessary. He had watched, and indeed had been " largely instrumental in providing the facilities for its study both in India and " in this country, on the new lines of the comparison of results for different " parts of the country or of the world. He was conscious that the new science " required a new method, that the method of the physical laboratory which aims " at elucidating a physical process by experiments specially directed thereto " was inapplicable to the case of the free atmosphere. Those who are critical " of the vast accumulation of meteorological data which is going on are apt to be " unaware of the fact that data have to be collected in advance, and that, to this " day, nearly every attempt to deal with a meteorological problem of any " importance is baffled by the want of data; they are equally unmindful of " another noteworthy fact, namely, that in meteorology comparison is of the " essence of the science. The meteorologist is absolutely dependent upon other " people's observations; his own are only useful in so far as they are comparable " with those of other people. Thus the time, trouble, and money spent upon " organisation are not the expressions of limited scientific ambition, but a

" primary condition for securing indispensable facilities. Strachey's scientific " judgment was extraordinarily acute. He was quite prepared to carry on " investigation to a speedy issue when circumstances permitted, as in the " investigation of the Krakatoa eruption, which led to the recognition of a " westerly drift in the upper air of the equatorial regions as a primary " meteorological datum."

The paper to which Dr. Shaw here refers is Strachey's admirable contribution to the volume of the 'Philosophical Transactions' of 1888, on the barometrical disturbances and sounds produced by the eruption of Krakatoa. To revert to our quotation:—

" In dynamical meteorology he was convinced that the most promising " mode of attack was not to look for a direct dynamical explanation of the " striking features, the eccentricities of the day's weather, which are the " almost fortuitous result of many causes combined in various phases; but to " seek for the relations between regular sequences and their causes underlying the apparently arbitrary variations. For this reason the methods of " harmonic analysis specially attracted him, and he was disposed to regard " anything less general than five-day means as unmanageable. He never " completed the work on harmonic analysis that he had in hand. He " attached particular importance to the third Fourier component of diurnal " variation, because the length of the day in these latitudes oscillates between " one-third and two-thirds of the twenty-four hours. A few years ago he " took up again the investigation of the question, and he has left a considerable amount of unfinished material.

" He was not to be driven from a position of modest optimism about such " matters, and always explained that for a new science the progress made in " the last fifty years is quite as great as could fairly be expected.

" But he was no friend of the unnecessary compilation of data or of the " unlimited extension of mean values. Almost the last contribution that he " gave me was a computation of the number of years necessary to reach " a mean value for temperature within the limits of the probable error of the " mean value for a single year, based upon some tables published in 1902 for " the extrapolation of mean values. He was always more concerned to present " meteorological data in a form amenable to computation than to increase their " volume or detail. When the weekly weather report was initiated in 1884, " he provided formulæ for computing the true daily mean from the maximum " and minimum temperatures for the day, and for computing the amount of " effective and ineffective warmth as referred to a base temperature of 42° F., " which are still in use. He once astonished me by pleading for graphical " representation as being easier to read than columns of figures, for he could " extract the meaning of a page of figures with a facility that made the discussion of results with him an indispensable part of any piece of work that " was in hand. Yet he was more than eighty years of age when we had to " transact this kind of business together. He never lost his appreciation of " new methods which were sound, or of new projects which were promising.

" Throughout his administration of the office he held to a high scientific ideal " while maintaining the efficiency of such daily work as was required for " public use and for international co-operation. His scientific horizon was " a wide one. With Stokes and Balfour Stewart he was largely instrumental " in providing means for the organised study of the sun, which had been " commenced in this country and in India by Sir Norman Lockyer, in order " to trace the primary causes of those great meteorological fluctuations which " exhibit themselves in alternations of drought and plenty in India, a study " which, pursued for many years at the Solar Physics Observatory at South " Kensington and at Kodaikanal, in India, has recently taken its place " among the greater international organisations. As head of the Public Works " Department in India he transferred meteorological work in that dependency from a provincial to an Imperial basis under Blanford and Eliot, and " laid the foundation for the admirable organisation of which the Government " of India and its scientific staff now enjoy the advantage. At the same time " he initiated the forestry department, and the application of botanical science " to the service of the public in that department.

" Probably no single person had clearer views of the future that lies before " meteorological work as a matter of practical influence upon everyday life, or " was more fully conscious of the long years of observation, organisation, and " study that are necessary to secure the advantages which will ultimately " more than reward the long years of patient inquiry."

On his retirement from the Meteorological Council in 1905, his colleagues on the Council addressed a letter to him expressive of their sense of the advantages which had been secured to the meteorological service of the country by means of his Chairmanship of the Council. In his acknowledgment he wrote thus :—

" The exceptional difficulties that surround the scientific treatment of the " subjects which the Council has had to consider have been further increased " by the restricted means at our disposal for dealing with the great diversity " of the objects that called for attention; and it is no small satisfaction to " me to feel that it has been possible for us to do so much, and to maintain " a scientific level that is, to say the least of it, in no way below the standard " attained by similar institutions in other countries carried on under far more " favourable conditions.

" The success thus secured is certainly due in no small measure to the " hearty co-operation of all the members of the Council and their Secretaries, " supported as they have been by a highly intelligent and devoted staff, " several of whom have been connected with the Office from the time of its " original constitution.

" Conscious as I am of my personal limitations, I thank you most " sincerely for the generous appreciation you have accorded to my efforts, " and specially for recalling my association in the work of the Council with " the former eminent members whom you have named. To these I may be " allowed to add two of our distinguished retired members, Francis Galton,

" to whose fertile genius meteorological science owes so much, and Admiral " Wharton, through whom the active association of the Hydrographic Depart- " ment of the Admiralty with the Marine branch of the Meteorological Office " has been so greatly promoted."

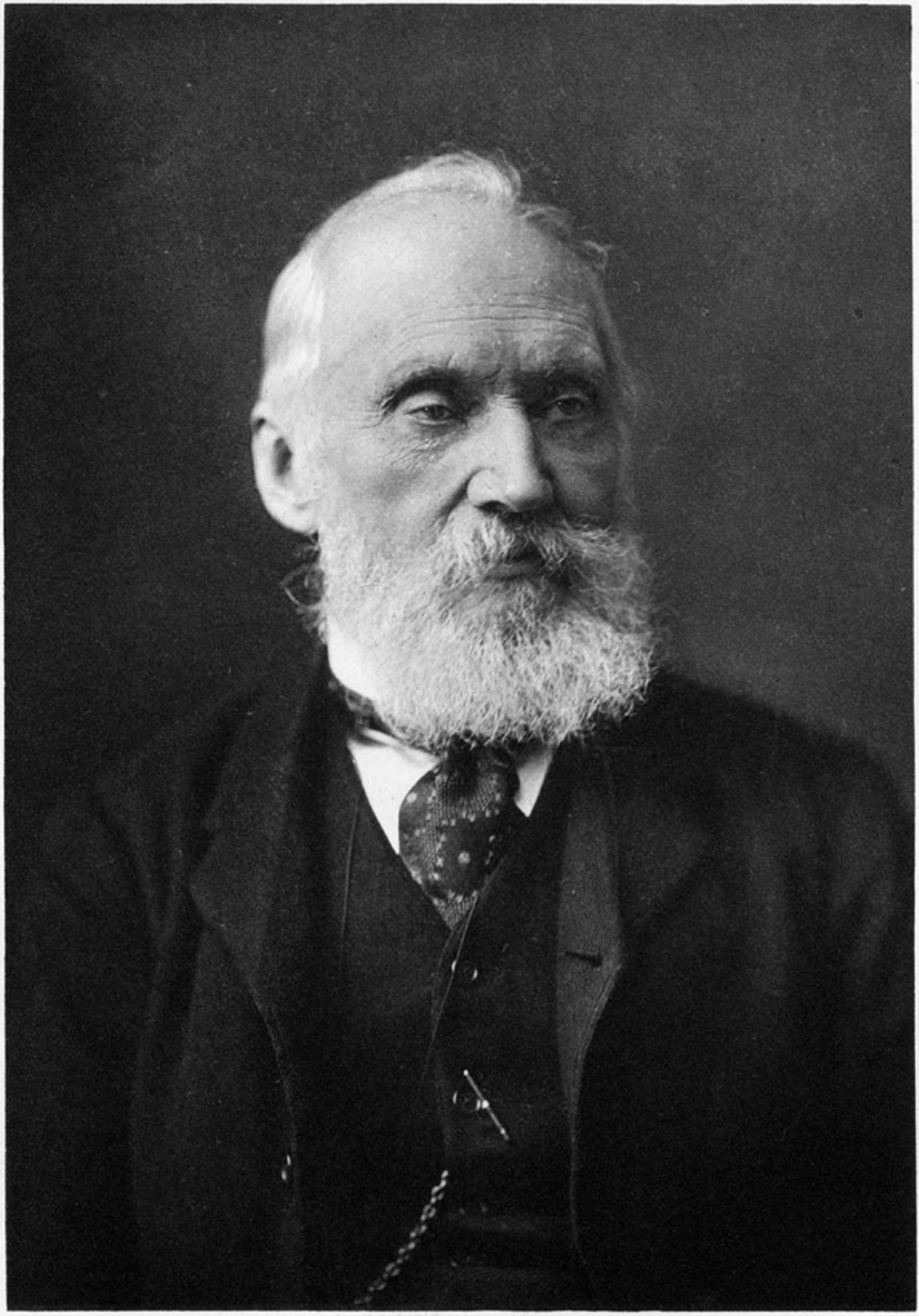
In 1866 Strachey was made a Companion of the Star of India, and in 1897 he was advanced to the highest grade of the Order, the Grand Commandership. In the same year one of the Royal Medals of the Royal Society was awarded to him for his researches in physical and botanical geography and in meteorology. Finally, in 1906, he received the Symons Medal of the Royal Meteorological Society.

After his return from India he lived for some time at a house with a charming garden, called Stowey House, on Clapham Common. Later he moved to a house in Lancaster Gate, and, only a few months before his death, he again removed to a house at Hampstead. Almost every summer he was accustomed to take a furnished house somewhere in the country, usually in one of the home counties. Although these times were nominally holidays to be enjoyed with his family, he still did much work, and usually attended various meetings in London. During the last two or three years he had several severe illnesses, and on one occasion, when nearly eighty years of age, he was knocked down by a cab in the streets of London. His excellent constitution enabled him to rally wonderfully on these occasions, and he was soon at work again. After he gave up all his many positions, and when he became less vigorous in health, he principally devoted himself to reading novels, but his mind remained wonderfully fresh to the end, so that he still took an interest in all that was going on in the world. His final illness was an attack of influenza, from which he had not sufficient strength to rally. He died on February 12, 1908, being then nearly ninety-one years of age. A memorial service, held at Christ Church, Lancaster Gate, was largely attended by many of his old Indian colleagues, and by representatives of many of the learned societies.

Although no attempt has been made in this article to speak of Sir Richard Strachey's family life, it is proper to mention that in 1859 he married Jane, daughter of his old chief, Sir John Peter Grant, of Rothiemurchus, who survives him. He leaves also five sons and five daughters.

He was in stature slightly below the middle height, and was somewhat shortsighted. His appearance was striking, and he always retained the look of a soldier; in conversation he was invariably interesting, and one could not fail to be impressed by the vigour of his common sense and by the incisiveness of his views. It was a privilege to know one who combined in so rare a degree the practical energy of the great administrator and the insight of the man of science.

G. H. D.



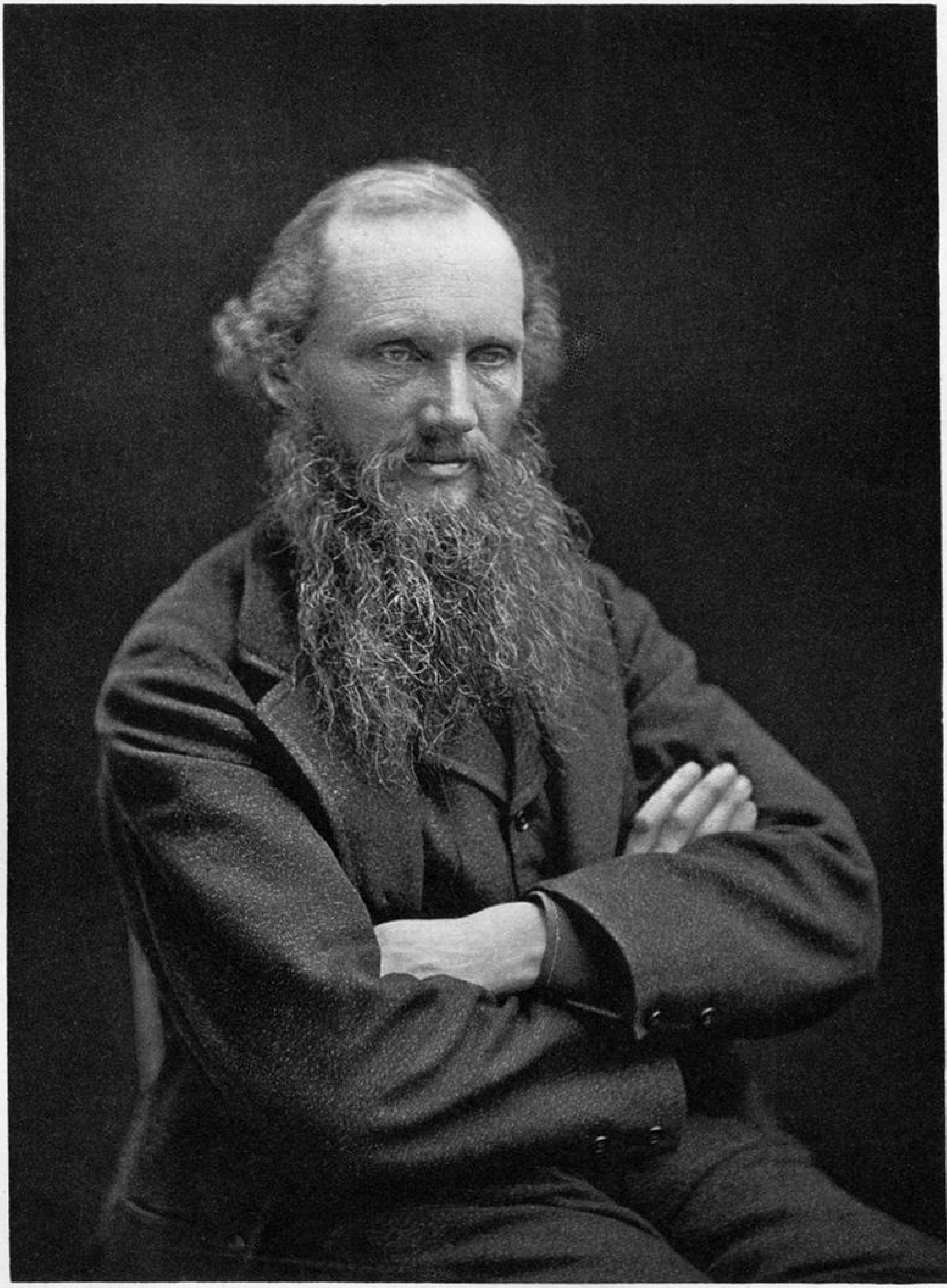
Yours very truly
Kelvin

1907

From a Daguerrotype.

Yours always truly

1854 William Thomson



Yours very truly
William Thomson

1877